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MEDICAL AND PHYSIOLOGICAL

COMMENTARIES.

BY

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> ——Vitæ tam vires quam actiones expono.—*Biblia Naturæ.*
>
> Morborum quoque te causas et signa doceo.—*Virgil.*

IN THREE VOLUMES.

VOL. III.

NEW-YORK:
PRINTED FOR THE AUTHOR.
1841—1844.

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PREFACE TO VOL. III.

It was the original intention of the author to have bound with the former volumes of the *Medical and Physiological Commentaries* the pamphlets which he has published in their defence against the criticism of two distinguished European Medical Reviews. Other publications by the author having, however, multiplied, and all of them being relative to the objects of the Commentaries, he has determined to present them to the public as a third volume of that work.

The more the author has deliberated upon the justice and propriety of thus collecting his defensive essays, the greater have become his reasons for adhering to the design, as expressed in these papers, of incorporating them with the work assailed, "that a proper issue may obtain between the reviewer and the author." (*See Examination of a Review*, &c., p. 66.)

The author will add, however, that his object in embracing in this volume the "*Reply to H. I. B.*," relates exclusively to the author's extended remarks on the writings of M. Louis. The author finds an apology for the "*Attack*" in the personal regard of its writer for his distinguished friend, and in other private considerations.

Four years have passed since the publication of the two volumes with which the present is now connected. During that period the author has been laboriously employed in the investigation of the several topics whose discussion makes up the Commentaries; has prepared for publication a large work on the Institutes of Medicine; has delivered three courses of lectures upon the Institutes; has carefully reviewed the Commentaries themselves, and has been actively engaged in professional business. With this additional experience and information, and with all the light of criticism, and the march of chemistry, before him, the author avers that there is not one line in the Commentaries which he would wish to erase or to amend; with the single exception of a verbal error which is noticed in his "Reply to H. I. B." In respect to chemistry, the author may safely affirm that not a fact has been subsequently disclosed that reflects the smallest light upon physiology or pathology. The whole of that ground, wherever chemistry had obtruded itself upon the science of life and disease, is so amply explored in the Commentaries, that not a substantial fact, nor a vague conclusion, has been put forth by the school of Liebig, which is not there examined, anticipated, and answered, as something which had already an existence, or was likely to emerge from the speculative philosophy of the laboratory then in almost universal vogue. That this is not the author's opinion alone appears from numerous admissions that have fallen from the press; of which the following may be quoted, as one example, from the distinguished author of the "Climate of the United States and its Endemic Influences," and editor of the "New York Journal of Medicine and the Collateral Sciences." Thus:

> "Dr. Paine, it will be seen, in fact, anticipates the whole chemical theory of Liebig, as set forth in his *Animal Chemistry*. This he does not only in his *Essay on Vitality*, in which he controverts some of the German professor's opinions, advanced in the *Organic Chemistry applied to Agriculture and Physiology*, but likewise in his *Medical and Physiological Commentaries*, published before the appearance of either of Liebig's works."

New-York, *June* 20, 1844.

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ERRATA.

IN EXAMINATION OF REVIEWS.

Page 76, two last lines, for *he had said*, read, *the reviewer has fabricated.*

Page 77, second line, for 6, read 7.

Wherever the word *Reply* occurs in this *Examination*, as referring to other places, substitute *Examination.*

IN NOTICE OF REVIEWS.

Page 3, line 18, for *complete*, read *complex.*

Page 3, line 40, for *Platonic*, read *Plutonic.*

Page 3, last line, for *constituted*, read *constitutes.*

Page 7, line 32, for *miracles*, read *miracle.*

IN THE INTRODUCTORY DISCOURSE.

Page 33, line 4, for *vigorous* read *rigorous.*

AN

EXAMINATION OF A REVIEW,

CONTAINED IN THE

BRITISH AND FOREIGN MEDICAL REVIEW,

OF THE

MEDICAL AND PHYSIOL. COMMENTARIES,

BY THE AUTHOR,

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NEW-YORK:
HOPKINS & JENNINGS, PRINTERS,
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1841.

EXAMINATION OF A REVIEW, &c.

In the April Number, 1841, of the *British and Foreign Medical Review*, a writer bestows a long notice upon the Medical and Physiological Commentaries, which calls for a reply from their author. He will take up, and nearly in their order, the principal points of objection, and it will then be seen what are the most vulnerable parts of the work, in the opinion of the distinguished Journal, and how far the "Commentaries" are correctly represented.

It should be premised that the reviewer is opposed, *toto cœlo*, to the cardinal doctrines of the vitalists; and it may appear that some private considerations have also lent their influence.

The review opens with the declaration that "a great book is a great evil," in the case before us. But, was not the length of the Essays necessary to the facts and the illustrations they embrace, and were not the facts and illustrations necessary to questions so long and ably controverted? Are they not the very means which have discouraged the reviewer from all attempt to show that the doctrines may be successfully impugned? Horace, who advocated brevity, allows that on all difficult questions, the most trifling circumstances should not be neglected when they tend to multiply the points of view; and the author had in his way a host of obstacles which it was necessary to surmount. To remind the reader that all may not as readily "grasp" the merits of great and controverted questions as himself, especially where the author was almost "single handed," (Vol. I. p. 391,) the precaution was taken of quoting from Zimmermann the following incontrovertible truth, namely:

"'As it is impossible to arrive at the knowledge of a whole, before we are acquainted with its parts, it will easily be conceived of how much consequence it will be not to neglect the least circumstance, even that which seems the most known. This known circumstance is, as it were, the *chain* that unites together the truths we are in search of. It draws us nearer to the unknown, and enables us to see nature more nearly.'" The Commentaries then remark, that "this is our apology for any seeming prolixity, either here or elsewhere." (*Comm.* vol. i. pp. 492, 531, 391, 267; vol. ii. p. 300.)

It is clearly absurd to suppose that subjects of such magnitude as are examined in the "Commentaries," where the arguments of so many profound writers were to be met, and where so many facts must be arrayed in opposition, could have been despatched with that brevity which might be most convenient for a Review that professes to survey the whole field of foreign and domestic medical literature. The author had been too much admonished by the fate of others, and was too respectful of others, to neglect the available means of sustaining his position; and the very course which has been taken by the reviewer is ample proof that the author acted wisely.

To illustrate the advantages of brevity, the reviewer institutes a comparison between Dr. Holland's "Medical Notes and Reflections," and the "Medical and Physiological Commentaries." Of the former, it is said that "the sketches are drawn by the hand of a master, though mere *outlines*;" and, in farther opposition, they are said to "convey a greater number of clear ideas to a reader of *ordinary* capacity than the most highly-wrought of Dr. Paine's finished compositions." "The latter contains a mass of materials, indeed, of great learning," &c. Now, in the first place, as already said, the author was writing not alone for "readers of *ordinary* capacity," but for all sorts of capacities, and in view of a torrent of opposition which he well knew must be encountered.

The reviewer has thus forced upon his author the necessity of involving Dr. Holland in this controversy; and if the reader will turn to the "Commentaries," Vol. I. pp. 396, 543, he will find what coincidence of opinions with Dr. Holland suggested the commendation bestowed upon his work at the expense of the "Commentaries." The author of the latter will also subjoin in a note some of the leading doctrinal views of the gentleman with whom he is contrasted, that it may be readily seen what are the reviewer's opinions of "just and vivid conceptions," "power and learning," "clear ideas," "lucid views," and other criteria by which he would have the "Commentaries" tried. It is worth remarking, also, that, in respect to *design*, there is not the *most remote* parallel between Dr. Holland's work and the "Commentaries." The former professes nothing but to arrange what was already known or supposed to be known, without attempting to show the truth of what was controverted. (¹)

But suppose an author were to expunge according to the taste, or the learning, or the prejudice, of his several readers, how much of his work would probably remain in the end?

Besides the *nine* principal Essays, the "Commentaries" embrace *fifteen* distinct, and elaborate Appendixes, each upon specific questions, and of difficult investigation. How generous, and how respectful of facts, is the following statement, the author submits to the reader, namely: —

> "Of the degree of elaboration our readers may judge, when we say that Dr. Holland's single volume of six hundred pages contains *thirty-four* essays, whilst in Dr. Paine's bulky tomes we find but *nine.*" (*Review*, p. 383.)

But, what if it were so? Would it prove any thing more than a similar comparison with a medical dictionary?

The reviewer's next objection is thus expressed: —

> "To show that a certain *law* is not capable of explaining *all* the phenomena of a given action appears to be regarded by Dr. Paine as quite sufficiently proving that the *law* had no bearing on the action." (*Review*, p. 383.)

There is nothing in the "Commentaries" that can possibly suggest so great a perversion of the author's opinions. The statement is entirely predicated of the reviewer's physical doctrines of life, and of the author's opposition to them. The latter fully concedes that he does not admit the participation of physical laws in any "given action" of organic beings, — unless they be of such a nature as concern the *motion* of the blood, abstractedly considered. The power which generates the motion is the *vital* power. But, the illustration of the reviewer which immediately follows the foregoing quotation, is neither to the point, nor does it appear to bear out those "relations of ideas" in which the reviewer is pleased to consider the "Commentaries" sometimes deficient, —espe-

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(¹) Dr. Holland is so eminently a humoralist, that he believes that "the blood may take on morbid conditions directly transmissible to offspring." "The accumulation of this matter of the disease," (gout,) he says, "may be presumed to be in the blood," and he thinks that *colchicum* can only act in three ways; either by "destroying the matter of gout by some specific change;" or, by "withdrawing it from the part affected into the general circulation;" or, by "procuring its removal from the system, through some of the excretory organs." "Collectively, they seem to include *all the modes* in which the medicine can act." Not a word of its possible action upon the diseased solids.

There is also a long disquisition upon "the Hypothesis of Insect Life as a Cause of Disease." A brief extract will serve the writer's purpose in respect to the reviewer. Thus, in adopting the opinion of the insect origin of the malignant cholera, among the reasons assigned, we read that, "in the hypothesis of production by certain animal species, *minute beyond the reach of all sense*, we have an explanation of the fresh creation and diffusion of the material cause," and especially as "out of the direct *dominion of the microscope* these animals are removed." Again, the "hypothesis" supposes these imaginary insects "capable of exciting vibrations in the air, of which man's grosser hearing is wholly unconscious; but which, received by their *fine organs* as audible sounds, minister to purposes of enjoyment and activity among beings *unperceived* by any of the human senses." It is also stated by Dr. H. that "Reamur and other naturalists have conjectured" that the clouds are sometimes made up of these insects. — *Dr. H.'s Med. Notes and Reflections*, pp. 15, 116, 122, 125, 126, 127, 135, 374, 524, 585, 562, 574, 576, 577. 1839.

cially when they do not acknowledge the intimate relation between the laws of organic and inorganic beings. Thus, the Journal in its *very next* sentence:—

> "Now, any one who has given the slightest attention to the science of life,—Biology,—must be aware how little any phenomena exhibited by living beings can be attributed to the UNDISTURBED operation of any single *law*; and how constantly the physiologist and pathologist are obliged to make allowance for *circumstances* which interfere with the operation of the simplest and best understood *principles*." (*Review*, p. 383.)

This is all very true, and constantly kept before the reader in the "Commentaries." (See quotation from Commentaries forward.) But,—what, pray, have the contingent "*circumstances*," which constantly "affect the operation of the simplest and best understood principles" in physiology and pathology, to do with the "law" as expressed in the first quotation? The last is designed as an exemplification of the first; but, where is the parallel between the "law" and the contingent "circumstances," "the undisturbed operation"—where "the relation of ideas"?

The reviewer being mainly *personal* on the subject of the microscope, the author will say but little in the way of defence. There is one remark, however, in connection with this subject, which serves to illustrate the reviewer. It is this:—"Dr. Paine forgets that, faithfully employed, it [the microscope] *can only disclose facts*." This the author denies, and has brought a mass of evidence which the reviewer does not attempt to impugn, unless by general "assertion." "But facts," he says, "are stubborn things." This is as readily granted,—but what does it prove? Has the reviewer attempted to show that the author has not proved the worthlessness of the supposed "facts?" It is true, there is a quibble about the author's satirical representation of the mode of estimating the weight of one of the supposed fossil animalcula, which is represented by Ehrenbergh as being "the 187,000,000th, part of a grain;" but what connection has this with the author's great object of showing the inapplicability of the microscope to physiological investigations? And, as to the general contradiction, the reviewer shall have the advantage of it in his own language. Thus:—

> "He quotes a number of instances in which microscopic observers have differed," "but he does not tell us *how completely*, with instruments much improved during the last few years, and with increased knowledge of the circumstances which can affect the accuracy of the observation, *all good microscopists are now agreed on these*, as upon a multitude of other interesting and important questions." (*Rev.* p. 384.)

It is very true the author has done no such thing, and for the reason that he has shown *exactly the reverse* of what is represented by the reviewer, and of the most able microscopists of the present day. But this is a small circumstance compared with other perversions.

Finally, the author has quoted, in his "Appendix on the Microscope," as greatly distrusting and generally *condemning* that instrument, *John Hunter*, *Dr. Bostock*, *Dr. Hastings*, *Bichat*, *Dr. Granger*, *the Medico-Chirurgical Review*, *the Edinburgh Medical and Surgical Journal*, *the Dublin Medical Press;* and, among others, the BRITISH AND FOREIGN MEDICAL REVIEW, 1837, which says, —

"'*Every one*, who has examined doubtful objects by a *high* magnifying power, must be aware how much is often left to the *imagination* of the observers; and it is not difficult to account for the GREAT DISCREPANCY which exists *in the statements of animal* as well as *vegetable anatomists.*'" (*Comm.* vol. i, p. 711.)

The reader will ultimately feel interested with knowing that something is said about *Dr. Carpenter* in this place, which is as follows: —

"The Dublin Medical Press, 1839, in speaking of Dr. Carpenter's *General and Comparative Physiology*, remarks that, — 'there is a chapter on the elementary structure of vegetables, and another on that of animals, in which, according to the *established usage*, the *improbabilities*, and *fictions* of the microscopic observers are recorded.' 'The student must not place confidence in these *wonders* of the microscope.' 'But, this is *entre nous;* for, if it were known that we entertained such heretical notions, in these palmy days of the reign of microscopes, we might risk encountering some grievous *bodily harm.*'" (*Comm.* vol. i, p. 711.)

Having disposed of the microscope, the reviewer begins a *pretended* defence of *Dr. Carpenter;* but, as this is subsequently resumed at great length, the author will delay its consideration, and take up the long and critical disquisition, which follows next, as to the proper import of the word "FORCE." This being the reviewer's great POINT, it requires at the hand of the author a profound consideration. The reviewer contends that the author is wrong in assuming a meaning for the word which is not recognized "*by philosophical writers in any department of science;*" and yet, as the author will now show, the reviewer employs it in the very sense which he condemns. But, let us first have the *manner* in which the reviewer gets at his premises. Thus: —

"Returning to our inquiry," says Dr. Paine, (vol. i, p. 12,) "'what is life?' and to the consideration of its definition by Bichat, and the philosophers of his school, — we consider the *functions* as being merely the result of peculiar forces operating upon organic matter, and that life virtually consists in the *co-existence of these forces and that peculiar substratum*. The *forces* are, to a CERTAIN EXTENT, in a *passive* state, WHEN NOT EXCITED BY THEIR APPROPRIATE STIMULI. But, they are still the essence of life; and whilst they *endure*, whether in an active or SEEMINGLY passive condition, life is constituted.' [The reader will ultimately see how all this is misrepresented by the reviewer.] Now here, Dr. Paine evidently recognizes the existence of a 'force' as something distinct from matter, on which it acts. In the previous paragraph he speaks of 'force' and 'property' as synonymous, [words referred to are "forces, or *properties*, if you prefer;"] and we are thus enabled to understand the idea which *he* attaches to the following definition, contained in a subsequent part of his essay.—'The foregoing considerations' he says, (p. 18,) 'would lead us to regard life as a cause, and to define it as consisting of certain specific properties appertaining to organized matter, which are more or less capable of resisting the destructive agencies of inorganic matter, and the forces to which it is liable, and of protecting against them the matter in which they are INHERENT.'" (*Review*, p. 385.)

Of this definition of life, it is then said, —

"We do not know when it has been our fortune to meet with a more complete jumble of ideas, than that which *would be involved* in the foregoing sentences, *if interpreted according to the usual meaning of the* WORDS *composing them.*" (*Review*, p. 386.)

The author will now not only defend his definition as perfectly consistent and accurate in all its parts, but will place his reviewer in the dilemma provided for the author. But, the *cunning* which characterizes the review must be first exposed. By a specious fraud the reviewer endeavours to make his author confound the forces of *life* with the *mechanical* forces, when an *absolute contradistinction* is observed between the former and all the forces of physics, throughout the "Commentaries." It will be ultimately seen that the author employs each denomination of force according to the acceptation of the best philosophers in the several departments of science. It should be kept in mind, also, that the reviewer's criticism *bears* mainly upon the author's definition of life, in which both the *vital* and *chemical* forces are spoken of. But the reviewer, to make out, as successfully as he might, the imputed confusion, diverts the attention of the reader from the author's reference to the *vital* and *chemical* forces, by starting off upon an *attempted* exposition of his own notions of *mechanical* force, and thus endeavours to impress the reader with the belief that the whole import of the definition, even of the *vital* "properties," violates the signification of *force* as employed "by philosophical writers on *any* department of science." As the author has said, this expedient is his reviewer's *greatest point*, since if he could persuade his readers of the imputed "jumble of ideas" in the author's *definition* of life, it might follow in the mind of the reader that the whole context was equally a "jumble."

A second great intention of the reviewer is to divert the reader from the author's argument, which relates entirely to the question whether life be constituted by "*the functions*" in their ordinary sense, or by the "*vital properties*," of which the "functions" are only a consequence. The reviewer substitutes "action of *some* kind" for "vital functions," and fixes his illustration upon *phys-*

ical force. As to *the* "action" of which the reviewer speaks, the author is in perfect accordance with the best physical and chemical philosophers, and goes beyond Dr. Carpenter and the reviewer himself in admitting the necessity of "*some kind* of action" whenever *vital* force may exist; as will be soon set forth. The author will now add, that the reviewer's criticism *turns* entirely upon insulating the expression as to the—"*forces of life* whether in an active or *seemingly passive* condition,"—the author thereby referring to those "functions" which are *appreciable*, and which, by the physiologists with whom the argument was conducted, are said to constitute life. (*See, also, the preceding sentence*, p. 7.)

The author will now proceed to show the stratagem, and, also, how the reviewer, whilst weaving the net, employs the term *force* in the very sense which he condemns.

1. "By philosophical writers," says the reviewer, "on *any* department of science, the term *force* is only applied *when an action* of some kind *is taking place;* thus, the *force* of gravity, that is, the *attraction* subsisting between the earth and the falling body *causes* the clock-weight to descend, or if checked, *occasions a pressure against* the impeding substance."(1) (*Review*, p. 386.)

Dr. Carpenter has the same illustration, but substitutes the word *power* for *force*, these being synonymous with him. Thus:—

"The *power* of gravitation *is called* into *exercise* when the clock-weight is wound up." (2) (Carp. *Princip.* p. 132.)

Now the reviewer is here right in his *illustration*, and obviously supposes that the *force* of gravity is equally *in* "*action*" (or "exercise") whether the clock-weight be at rest or in motion; which is the true philosophy of gravitation. Such being his meaning as to "action," the author fully agrees with him so far, as it respects all kinds of physical force. But, what connection has this with the author's remark in relation to the *forces of life*, which he has shown to be *inactive* in the *seed* as it respects all but *vital affinity*, and allowed to be perfectly passive, in the absence of stimuli, even by the reviewer and Dr. Carpenter. The reviewer goes on thus:—

2. "The Creator, in giving origin to that which we term matter, by that very act CREATED the FORCES *by which* different material bodies *operate* upon one another." (*Review*, p. 386.)

Now was there ever a more glaring contradiction, and "in the same breath"? Here we have positive *agents*, real "*entities*," something actually "*created*," *inherent* in matter and capable of "*operating*" on other matter,—since the reviewer will hardly maintain that when the Creator *creates* such "forces" as are here spoken of, he means that the Creator created *nothing*. These

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(1) The author takes the liberty, in common with his reviewer, of marking certain words in Italics, &c.

(2) Carpenter's Principles of General and Comparative Physiology, p. 132—1839.

forces, then, may be either in an *active* or *passive* state. Now let us have the next following sentence. Thus:—

> 3. "For *all forces result* from the *action* or *operation* of the *properties*, (thus differing *essentially* from the properties themselves) *with which* matter is endowed." (*Review*, p. 386.)

This is more a curiosity than a matter for grave criticism. In one sentence, we have real substantial *forces* inherent in matter, ever ready to operate, and the handy work of the Creator, whilst in the very next, the prerogative is snatched from the Creator and given to the "properties" of matter, which, of course, are very logically said to "differ essentially from" the forces which they create.

The author cannot help thinking that his reviewer has been again borrowing from Dr. Carpenter in this instance; for Dr. Carpenter says:—

> "In machines constructed to take advantage of the physical *properties* of matter, and to bring them into useful operation, a *stimulus* to their action is required, in some means which shall *develope these properties*, and thus *create* powers [or forces.] Thus, the *power* of gravitation is called into exercise, when the clock-weight is wound up." (1) (Carp. *Princip*. p. 132.)

Thus the winding up the clock is the "stimulus" which "develops" "physical properties," (already developed in matter by the Creator,) which properties, thus artificially *stimulated* and developed, then "*create* powers"! "Vitalists," even the "transcendental," have nothing comparable with this. But that it is really so will more fully appear from another paragraph from Dr. Carpenter, by which it will be farther manifest that the reviewer must have borrowed the whole of this explanation from Dr. Carpenter's work, since he agrees with Dr. Carpenter after the following verbal manner. Thus, Dr. Carpenter:—

> "The term Law of Nature, as already employed, expresses the *conditions of action of* THE PROPERTIES of matter. The Divine Creator of the Universe 'has, by creating his materials, *endued* with certain fixed qualities and *powers*, *impressed them in their origin* with the spirit not the letter of his law, and made all their subsequent combinations and relations INEVITABLE *consequences* of this *first* impression." (Carp. *Princip*. p. 134.)

And yet, in the same paragraph, Dr. Carpenter contradicts this fundamental proposition. Thus:—

> "To suppose, as some have done, that the *properties first impressed* upon matter would *of themselves* continue its actions, [yet '*inevitable consequences*,'] is to deny all that revelation teaches us regarding our continued dependence on the Creator. Let it be borne in mind, then, that when a *law* of Physics or *Vitality* is mentioned, *nothing more is really implied* than a simple expression of the *mode* in which *the Creator is constantly operating* on inorganic matter, or on organized structures." (2) (Carp. *Princip*. p. 135. — See *Comm*. Vol. I, p. 25, for the perfect accuracy of the author's statement.)

Here, also, the reviewer contradicts himself in the same manner, when ridiculing in Dr. Paine the very doctrine which both himself and Dr. Carpenter would have us *believe* and *disbelieve*. Thus: —

---

(1) And so the reviewer, thus: — "Does any *force* exist until the water has been converted into steam by the *stimulus* of heat, and the steam by a still further addition of temperature caused to expand with violence? Assuredly not." (*Review*, p. 386.)

(2) So thorough is Dr. Carpenter in this contradiction that he says in a note, — "*If miraculous interpositions* are exceptions to general laws, they are *so only* in human estimation." *Op. cit.* p. 135.)

> "Yet this is only carrying out Dr. Paine's doctrine in the mode he himself warrants, when he tells us, (Vol. I. pp. 10, 25,) that the Deity is *not* the *immediate* cause of the phenomena of nature, but that he has brought into existence a number of *forces*, which are the *proximate* causes of all phenomena." (*Review*, p. 386.)

Such *is exactly* Dr. Paine's doctrine *throughout* the "Commentaries," — nowhere *violated*, nor did he "mistake it," as the reviewer supposes, "for a new hypothesis."

The following, therefore, is the "relation of ideas," both with the reviewer and Dr. Carpenter: 1. Before gravitation can be "called into exercise" upon "a clock-weight," it must be "stimulated." 2. "The Creator *created the forces by which* different material bodies *operate* upon one another." 3. "All the subsequent combinations and relations of matter are the *inevitable consequence*" of the "*forces*" or "*powers*" so created by the Creator. 4. But, the actions and phenomena of organic and inorganic beings are not only *not* the "inevitable consequences," or "proximate" results of the "forces" or "powers," or even "properties" so created, but they flow *directly* from the "mode in which *the Creator* is *constantly* operating on inorganic matter, or on organized structures." 5. The Creator did *not* create the *forces* which he *did* create, but "*all the forces*" are *created by* "*the properties* with which matter *is endowed*." 6. But "the properties" *inherent* in matter have no existence till "*a stimulus*" is applied "which shall *develope these properties* and *thus create* powers" or "forces."

Such is the philosophy of distinguished opponents of vitalism. The reader will observe that the summary corresponds exactly both with the reviewer's and Dr. Carpenter's statements, between which the coincidence is precise. The author has kept them distinct by appropriating to each writer a particular type. It was quite unnecessary, however, to have preserved this individuality, since it will appear that the reviewer agrees with Dr. Carpenter in all "his positions," and, through many pages, so mixes himself up with Dr. Carpenter, that it is extremely difficult to separate Dr. Carpenter from the reviewer.

If we now revert to the quotations from the review, marked 2 and 3, by putting these two consecutive sentences together, the reader will perceive that the reviewer also convicts himself of identifying "*forces*" and "properties," notwithstanding his simultaneous attempt to make one the "creators" of the other. The sentence number 2 is also a perfect recognition of the doctrine, which the reviewer violently condemns in this place as being advocated by Dr. Paine, "that the Deity is *not* the *immediate* cause of the phenomena of nature, but that he has brought into existence a number of *forces* which are the proximate causes of all phenomena." And, we have just seen by a quotation from Dr. Carpenter, that he also sustains this opinion of Dr. Paine, and yet declares it otherwise. The reader will farther observe that, in the sentence where an attempt is made to contradistinguish *forces* from *properties*, the reviewer renders *his* properties, (and, also, in the sentence about the clock-weight, his *force* or power of gravity,) *real* agents, — *acting*, *creating*, *&c.*

Again, take the following paragraph, in which the reviewer attempts a distinction between *properties* and *forces*, but, in doing so, recognises the existence of *force* as an *active power* like the *properties*, and, not only so, he again makes the *properties*, in opposition to himself, (sentence number 3,) the *creators* of the *forces*. Thus: —

> "Having disposed of *forces*, we will turn for a short time to *properties*. And, we would ask what other notion of matter do we possess than that derived from those *properties* which either directly affect our senses, through the material changes they produce in our organs, or which, by the influence of different bodies on one another, *give rise to forces that produce phenomena* of which we in like manner take cognizance?" (*Rev.* p. 386.)

"A writer," as the reviewer says, "who can so contradict himself, scarcely needs to be exposed by us." (This passage will be soon repeated in its application to the author.) The author has said that the whole of the foregoing explanation of "*force*," on the part of the reviewer, must have been taken from Dr. Carpenter's work. And so it must, — for by no fortuitous reverie could two minds have brought about a coincidence so replete with internal proof of a common origin. The "interpretration" is *unique*, and cannot be imitated. And so we shall see of other instances.

Having now taken the question out of the confusion in which it has been involved by the reviewer, and got at its true merits, let us see, and for other substantial purposes, how far the author is sustained in his imputed use of the term "*force*" or "*forces*," as they are employed in *physical* and *chemical* science.

For the foregoing purpose the author will first take the united authority of Laplace, and Professor Graham of the London University, and will, also, subsequently confine himself to "philosopical writers" who are justly held in the highest esteem by the British Review. The author has selected the following out of many similar passages, as expressing exactly the import of the word *force* as carried out in the "Commentaries," in its *physical* sense, and as conveying precisely the meaning in which it is employed in that part of his *definition of life* which relates to the *chemical* forces. Thus, Laplace and Professor Graham: —

"This supposition is conformable with the views of corpuscular philosophy entertained by Laplace. According to that profound philosopher, the FORM *of aggregation* which a body affects *depends* upon the mutual relation of *three forces*: 1. The *attraction* of each particle for the other particles which surround it, which induces them to approach as near as possible to each other. 2. The attraction of each particle for the heat which surrounds the other particles in its neighbourhood. 3. The repulsion between the heat which surrounds each particle, and that which surrounds the neighbouring particles, a force which tends to disunite the particles of bodies. When the first of these *forces* PREVAILS, *the body is solid.*" (1)

Now in the last of the foregoing propositions, or when "the body is solid," that "kind of action is taking place," which, by the reviewer's illustration, happens in respect to the "*force* of gravity" when a body is at rest upon the earth, and by which he so *totally perverts* his author in substituting that "*kind* of action" as his author's meaning in his argument as to the ordinary "vital functions." The reviewer's "*kind* of action," however, the author maintains must exist even in the "seed," as he will ultimately show, and as *fully* implied in the extract, page 7. But the author also maintains that it is a *consequence*, not a *cause* of life, as held by Dr. Carpenter and "other able writers on the same side."

The doctrine, as stated in the foregoing quotation, is frequently expressed in the "Commentaries." Thus, in distinguishing the vital from the physical forces, the author says, —

"Who shall say, that the fall of a stone, the motions of the planetary system, &c. more clearly evince their dependence upon the power that is called gravitation? The ocean rises and falls; but, this is only another coincident effect, and brings no variety to illustrate the force. The want of greater variety leaves the mind in doubt; and it is nothing but these simple effects, which imply a cause, that satisfies all that *some unknown* force PREVAILS. [The same word as used by Laplace.] But its *nature*, whether material or immaterial, an *emanation* or an *oscillation*, *heat* or *cold*, *light* or *darkness*, *no one can divine.* Still there is something, — a sort of spell, which HOLDS *all* MATTER, in one uniform way, under its CONTROL, — save only, in its proper sense, the living organized being. And here we see the *forces of life contradistinguished* from the *physical*, as they are from the *chemical*, — since the forces of life may partially overcome the force of gravitation. The man walks, — the bird flies. It will not, of course, be said, that so also does the bow send forth the arrow, and the powder the ball, — for there is no analogy in the cases." (*Comm.* vol. i, p. 44.)

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(1) Graham's Elements of Chemistry, p. 65, 1837.

"*The forces of life are inherent* in every part of living matter, and may *forever operate per se*, and resist all other [natural] forces. But, in all the other instances, as elasticity, expansion, and all [natural] mechanical forces, they are not the sources of actual power. *Self-action*, then, is a *distinguishing* characteristic of animals and vegetables; for the principle reaches *every part of their organization.*" A plant droops and rises again by its vital power. (*Comm.* vol. i, p. 45.)

What the author has said of our knowledge of the *force of gravity*, he has also often affirmed of the *chemical* and all other inorganic forces, — and nothing but the gross misrepresentation of the reviewer upon this fundamental point could have induced the author to prolong this discussion. In contradistinguishing the *chemical* forces from the *vital* properties, the author, among other general remarks of a similar import, has the following:—

"In respect to the *forces of chemistry*, is it known with any certainty, what particular causes are instrumental in effecting the chemical union of different species of matter? Are there not most remarkable revolutions in opinion as to the nature of magnetism, electricity, heat, and light, and the laws by which they are governed; and is not the doctrine of oscillations liable to be supplanted by another, to-morrow? Indeed, at this moment, there exist the theories of emanation and undulations of light. But, whether one or the other be true, it affects in no degree, our knowledge of the laws of reflection, refraction, &c., by which the specific existence of light is ascertained. And, although the phenomena of light are thus diversified, and present, as it were, a variety of laws, we know less of [the nature of] light than we do of darkness. Finally, *all that we know* of matter, [organic or inorganic,] and of the sources by which this knowledge is acquired, *arises from* effects." (*Comm.* vol. i, p. 45.)

Let us now hear Professor Daubeny, of the Oxford University, as to the nature of *force*, and how with the author, he supposes the *chemical* forces to be resisted by *forces* peculiar to the *living* body. The Professor has lately published a work in which he derides all the doctrines of the vitalists, and to whom, therefore, it may be presumed the reviewer will have no objection.

"The putrefaction of vegetable and animal matter," says Daubeny, "appears to be produced, not by any sudden cessation of those *affinities* which had previously bound their respective elements together, but by the predominance over them of *other* natural *forces*, which we may, without much difficulty, conceive to have been *controlled* under the circumstances in which the *liv-*

ing body is placed." (1) *Graham.* — That is to say, "controlled" by the *vital* forces.

Take up any author on chemistry and you will find the same thing. Here is Professor Turner, who says, —

"The tendency of cohesion is manifestly to bring the ultimate particles of bodies into immediate contact; and such would be the result of its influence, were it not *counteracted* by an opposing *force, a principle* of repulsion, which *prevents* their approximation." Nay, *vis inertiæ* itself is almost admitted among the *forces.* "The indifference of matter," says Turner, "to either state has been expressed by the term *vis inertiæ*, as if it depended on some *peculiar force resident in matter;* but it *arises*, rather, from matter being absolutely passive, and thereby subject to the influence of *every force*, which is capable of *acting upon* it."(2) — *Turner.* (Compare this with the author's definition of *life.*) N. B. The foregoing may apply to *mind* as well as to matter.

And now for the *dictionary.* "FORCE, L. *fortis.* That physical property in a body which *may produce* action or *motion* in another body, or may counteract such action." "*Mechanical* force, is the power that belongs to bodies *at rest* or in motion. The presence or tension of bodies *at rest* is called a mechanical force, and so is the power of a body in motion." (3) — *Webster.*

The author will now pass on to distinguished and recent physiologists as to their use of "vital principle" and "organic force," from which it will appear that the author employs the terms in their acceptation. Thus, Dr. Carpenter: —

"The terms *vital principle* and *Life*," he says, "are *commonly* employed almost synonymously, to imply the *controlling agent* by which the phenomena of living beings *are directed*, if not *immediately produced.*"(4) — *Carpenter.*

And now as to "*Organic* FORCE." Thus, Müller: — "There is in living organic matter a *principle* constantly in action, the *operations* of which are in accordance with a rational plan, so that the individual parts, which *it creates* in the body are adapted to the design of the whole; and this it is which distinguishes organism." "The organic *force* is manifested only in the organic compounds produced in those bodies. The *mere accidental coming together* of the elementary components *is not capable* of producing organic matter." "Even animals themselves have not the power of generating organic matter out of simple inorganic elements or *binary* compounds." He also calls it the "*creative force.*" Thus;

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(1) Quoted from the British and Foreign Med. Rev., Jan. 1841, p. 136.

(2) Turner's Elements of Chemistry, *Introduction.* 1835.

(3) Webster's Dictionary.

(4) Principles, &c. p. 131.

— "The *creative force exists* already in the germ, and *creates* in it the essential parts of the future animal." It is also a "creative *power*." "The formative or organizing principle is a *creative power*, modifying matter blindly and unconsciously." — *Muller.*

Müller, indeed, goes far beyond the present writer or Mr. Hunter, in supposing the "organic force" capable of rapid motion, — rapid as thought itself; and he also says it is capable of "multiplying itself," and "exerts its influence even beyond the surface of an organ, as shown by its effects on the chyle, in maintaining the fluidity of the blood," &c. "The *Vital Principle* is in a QUIESCENT state in the egg before incubation."(1) — *Müller.*

There is much of this in Müller, hereabouts, though he afterwards loses sight of it in pursuit of the "chemical forces" to which *he* ultimately gives as much importance as to the organic force.

And now Liebig, who has his *substantial chemical* forces as his "*vital* principle": —

"In vegetable physiology," says Liebig, "a leaf is in every case regarded merely as a leaf, notwithstanding that leaves generating oil of turpentine or oil of lemons must possess a different nature from those in which oxalic acid is formed. *Vitality*, in its peculiar operations, *makes use* of a *special apparatus for each function* of an organ. Vegetable physiologists, in the study of their science, have not directed their attention to *that part* of it which is *most worthy* of investigation." — Organic beings "constitute a form of reproduction *independent* of *chemical* processes. The *chemical forces are subject* to the *invisible* cause *by which* this form is produced. Of *the existence of this cause* itself we are made aware only by the phenomena which it produces. Its laws must be investigated just as we investigate those of the *other powers* which *effect* motion and changes in matter. The *chemical forces* are *subordinate* to this *cause* of life, *just* as they are to *electricity, heat, mechanical motion and friction.*" — *Liebig.* [Observe the contradistinction between "*chemical forces*" and "electricity, heat, and mechanical motion," which the reviewer confounds with "forces."] Again, says Liebig, —

"The *vital principle* is only known to us through the peculiar form of its instruments, — that is, through the organs in which it resides." — "In the same way the vital principle governs the *chemical powers* [same as "forces"] in the living body." — "If the food possessed life, not merely the *chemical forces*, but *this vitality*, would offer resistance to the *vital force* of the organism it nourished." — "*The vital principle opposes* to the continual action of the atmosphere, moisture, and temperature upon the organism, a resistance which is, in a certain degree, *invincible*."(1) *Liebig*.

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(1) Muller's Elements of Physiology, pp. 9, 18, 23, 25, 27, — 1838.

Now the only difference between Liebig and the author, whether it respects *vital* or *chemical force*, consists in the doctrine of the latter that, "there is no philosophy in the unmeaning multiplication of causes,—especially when the superadded ones will not explain a single phenomenon, or a single result appertaining to living organized matter." (*Comm.* vol. i, p. 99.)

But, where is the difference between the foregoing, and the "vital properties" and "vitality," (according to the mystifying school,) "making use of *chemical forces?*" (See examination of this subject in *Comm.* vol. ii, pp. 89, 121, 122.)

And thus Tiedemann: "Organic beings," he says, "are endowed with *powers* of a *particular kind*, namely, organic powers." "*All* the qualities of organic bodies should be looked upon as *the effects* of life. Even *those* phenomena seen in them, which they exhibit *in common with inorganic* bodies, *undergo modifications* of their specific action, and should be considered as *subordinate* to the organic powers." "Chemists *have not* succeeded [then nor since] in reproducing organic combinations, as they have done the inorganic compound bodies." "There must be in living bodies a *peculiar power*, differing from the chemical affinities, *which determines* the forms of bodies not endued with life, and the *action* of which produces the diversity which organic forms with similar composition exhibit." "Even when the *life* of organic bodies is *extinct*, we should consider the *qualities* which they possess, *from the time* of *death* to the *complete resolution* of organization, as results of the *organic powers* which *have been active* in them." [This is a doctrine of the Commentaries.] "Already," he adds, "it has been *more than once attempted to deduce life from the laws of mechanics, physics, and of chemistry. This error has been committed by the physiologists and physicians of the iatromathematic and iatrochemical schools. In every age, distinguished naturalists discovered this error and opposed it.*" (2) *Tiedemann*. Observe, "*peculiar power*" and "organic *powers*."

The vital principle or organic force is everywhere the presiding genius in the foregoing work, and is defended in all the attributes that are ascribed to it by the present writer.

And Bichat, too, whose "writings" the British and Foreign Medical Review laments, "are almost forgotten," (3) employs the word vital *force* in the same sense as the Commentaries, in a multitude of instances. It is his favorite substitute for vital properties. Thus, "the disengagement of *caloric* is always subordinate to the *state* of the *vital forces*." "The *vital forces*, especially the *tonic power*, have a very decided influence upon the extrication of caloric." And here is something for the chemical doctrine. "If chemists apply their theories to these *morbid changes* of heat, instead of considering them as a necessary consequence of the *state* in which the *vital forces* are then found, they will necessarily find in them an insurmountable obstacle." And here is something to aid the reviewer's conception of the author's philosophy of bloodletting and of the operation of other remedial agents. "It is *undeniable* that all remedies have for their object the *restoration* of the VITAL FORCES *to the natural type*, *from which they have been driven by disease*. Since the morbid phenomena may be considered as *different alterations* of these *forces*, the *action* of remedies should also be viewed as the means by which these alterations are to be brought back to the natural type." (1) *Bichat.*

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(1) Liebig's Organic Chemistry applied to Agriculture and Physiology, pp. 36, 355—358. London. 1840.

(2) Tiedemann's Physiology, pp. 7, 9, 28, 30.—1824.

(3) July, 1839, p. 186.

In the British and Foreign Medical Review "the vital principle" is also fully acknowledged, and the philosophy of the chemists as applied to organic beings and the humoral pathology condemned in some of the articles, and to which the present writer has referred (with the commendation to which the reviewer alludes, p. 401,) in his "Commentaries." (See vol. i, pp. 41, 530, 714 ; vol. ii, pp. 399, 415, 427.)

The author now comes to a more specific defence of his use of the term *force*, in its different acceptations when applied to organic and inorganic beings. In the first place, however, to avoid a *verbal* criticism, (which the reader will soon see extends beyond the word *force*.) the author defined exactly the meaning of several words, and that meaning is preserved, respectively, (and the author believes without a conflicting instance,) throughout the "Commentaries." An absolute distinction between the physical and vital forces or powers is fundamental in the work ; and when *force* is employed in relation to *organic* beings, it is always as defined. Thus:—

"The words *vital principle*, *vital power*, *organic force*, *organic power*, are used synonymously, and refer to the universal cause of animal and vegetable life, and are employed as collective terms. *Vital properties*, *vital powers*, and *vital forces*, signify the various *elements* of the *vital principle* or *vital power*, as they are manifested in *irritability*, *contractility*, or *mobility*, *vital affinity*, *sensibility* and *sympathy*. (*Com.* vol. i. p. 10.)

Again, "The forces of physics and of chemistry produce *precise and uniform* results; whilst those of life are attended by an *endless variety of modifications*. This is, in itself, an ample proof that

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1, Gen. Anat. Vol. II, pp. 44, 47, 49; Vol. I, p. 19.

their essence is *wholly distinct*. One set of forces operates *mainly* upon inorganic matter, and are *not acted upon*. The other forces operate *exclusively* upon *organized* matter, are more or less influenced by the former, and are *acted upon* by foreign causes. (See Turner's definition of force, p. 15.)

"The former are always the *same* under all circumstances; the latter exist in *distinct modifications* of *kind* according to the *varieties* of *texture* which they animate, and they are *constantly modified* in their *nature by an almost endless variety of foreign agents*, and they may be EXTINGUISHED by those agents. Their alterations constitute the *essential* pathology of disease. We may *calculate* the *results* of one with *perfect accuracy*;—we look with *prophetic* ken to the return of a comet through a long vista of ages. But INSTABILITY is the *great characteristic* of the *vital* forces, and the exact phenomena which we may contemplate *at this moment* may *vanish at the next, never to be repeated*. The *physical* and *chemical* forces are ALWAYS OPERATING *per se*, with *entire independence*, and *their existence is perpetual* as their nature is *unchangeable*. The *vital* forces, *on the contrary*, generally produce VISIBLE actions *only when* they are *acted upon*, and the *mind* itself is one of the agents. They become *more or less passive* the moment *those influences are* WITHDRAWN, and they may be EXTINGUISHED in a moment *by those influences*. (*See Reply*, pp. 5, 10, 13, 14.) They have a *specific final cause* which appertains *exclusively to organized matter*. This final cause, which consists in carrying on the processes of life, has a *natural end*, when the vital forces become EXTINCT, and *give place* to the *full operation* of the forces of *chemistry*. The latter *then lay waste* what had been the work of the former, and thus prepare new materials for the complex process of ultimately fitting them for the work of the vital forces in other animated beings.

"These are BROAD and IMPORTANT DISTINCTIONS. They place the respective forces IN OPPOSITION to each other. The distinctions indicate not only the nature of the forces, but the rank of all matter.

"Powerful efforts are making to *substitute* the forces of chemistry and other physical powers for those of life. We shall oppose this doctrine in all our future disquisitions; and it is therefore proper that we should, in this place, *point out the distinctions* as well as we may." (*Comm.* vol. i, p. 30—31.—Also, Reply, p. 5, 6.)

The author will subjoin, in a *note*, a farther illustration of the doctrines of the "Commentaries" as to the nature of "Laws," and with a particular reference to what is said in this *Reply* at page 5, 6. (1)

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(1) "We allude to GREAT VITAL LAWS, not to the ACCIDENTS of disease. Mr. Clendening asks, 'how should there be a rule in physiology, morbid or healthy, without exceptions?' In his acceptation of a *rule*, as illustrated by his subject, there is none; and we think it might be shown, in such instances, that the term is objectionable. Such general rules are *subject to great instability* from *accidental causes*, and they have no immediate connection with any of the *laws of nature*. But in respect to the great principles in physiology, we know of no exceptions in the natural state of the body; and this is, *prima facie*, a strong ground for analogical inference, as to the principles upon which morbid condi-

And again the author says, "We have already explained, that we are almost indifferent as to the *name* which the forces of life shall receive, so long as authors continue to apply the *same import* to more favourite words than 'vital principle,' or 'vital powers.'" (*Comm.* vol. i, p. 29.)

The foregoing are fundamental doctrines throughout the "Commentaries," and will illustrate other parts of the review besides the author's most perverted *definition of life*, which he will now repeat: —

"The foregoing considerations would lead us to regard life as a cause, and to define it as consisting of certain SPECIFIC *properties* [or forces] *appertaining* to organized matter, which are more or less capable of resisting the destructive agencies of *inorganic* matter, and the *forces* to which *it* is liable, and of protecting against them the matter in which they are INHERENT." (*Comm.* vol. i, p. 18.)

Now we have in the definition *two* kinds of forces, — namely, the *organic* force, and the *chemical* forces. The organic force, or "vital properties," according to the definition, is "more or less capable of resisting the destructive agencies of inorganic matter, and the forces to which inorganic matter is liable." That is to say, the organic force is capable of protecting itself, on all ordinary occasions, against the injurious influence of external agents, and the living body against the forces to which inorganic bodies are liable, — that is, against chemical decomposition, which would ensue in the organized body were it not for the protection of the organic force. (See Daubeny's and Turner's explanation, p. 14, 15.) But, to render this meaning obvious to an "ordinary capacity," it was added to the definition, "and of protecting against them (the destructive agencies, &c.) the matter in which they (the vital properties) are *inherent*." The reader will observe that *organic force* is sub-

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tions depend. Sooner or later the apparent exceptions are brought within the general *law*. Thus, it has been supposed that the circulation of the brain has a law peculiar to itself, and so, also another for the penis. But one of the conclusions is purely hypothetical, and the other is based upon the microscope, and has been shown to be an error."

Again, "When we are told that purulent matter is equally the result of a vital process, and of the putrefaction of blood, or of the solids, or is at one time generated by the action of the extreme vessels, and at another may form itself spontaneously in the blood; and that ulceration, according to M. Louis, and some others, may depend on *inflammation*, but again is owing to an '*opposite cause;*' and that the chemist may form the gastric juice in his laboratory, wherewith he imitates digestion, we consider such 'exceptions' to the *laws* of nature as an indefensible violation of 'general rules' which admit of no exception.

"The vital powers are certainly distinguished by an instability as it respects their liability to partial modifications; and it is upon this that much of the theory of medicine hinges. But they are always the same powers, affected in the same way, *cæteris paribus*, by the same causes, and leading to the same results according to their modifications respectively. It is the business of observation to ascertain what these changes are, and to record them as perpetual landmarks in the great field of Nature, where they may always stand, rescuing our science from the hands of mere empyricism, and guiding us under the name of general principles, which are as applicable, through all time, to the circumstances of any given modification, as others are to the most perfect integrity of the vital functions" (*Comm.* vol. i, pp. 626, 627 *note*.)

stituted, in this explanation, for *vital properties*, (which are synonymous with the author, and which occur in the definition,) that the substitution may meet the erratic criticism of the reviewer.

The reviewer, when employed in perverting the author's doctrine as to the "*organic force*," and "vital *functions*," and endeavouring, by turning the discussion upon the nature of *mechanical force*, and by substituting "some kind of (*physical*) action" for the author's argument as to "vital *functions*," *as well* as by more direct methods, to perpetrate the offence of making the author confound the *organic* with the forces of *dead* matter, artfully turns his physical illustration upon the author's expression, "The forces (of life) are to a certain extent in a passive state, *when* not excited by their appropriate stimuli," (Reply, p. 8,) after the following manner: —

> "What should we think of a person who should talk about a 'passive force' existing in *water*, by which are affected the mighty operations of the *steam engine?*" (1) — (*Rev.* p. 386.)

The foregoing expression of the author is the groundwork of the diatribe about the necessity of "*some kind of action*" when *physical* force is present. But, we have seen that the reviewer affirms that

> "By philosophical writers on *any* department of science, the term *force* is only applied when an *action of some kind* is taking place." (*Rev.* p. 386.)

Let us now present the subject according to the premises in the "Commentaries," which relate to "*vital functions*" of an *appreciable* nature, and it will be found that there is one "philosophical writer," at least, who thinks that what is denominated "vital force" by the most illustrious physiologists, may exist in a *perfectly passive* state, — without the least "action" of any "kind,"— and yet that *vital force* or "vitality" may be present. This author is *Dr. Carpenter*. But, in the first place, let us hear the reviewer a little farther, since he has so identified his own cause with that of *Dr. Carpenter*, it would be unjust in the author to hold them distinct in this mutual concern. For this purpose, the author must temporarily pass on to another subject, where the reviewer says that, —

> "The *onus probandi* rests upon those who maintain the existence of a *principle* which others declare to be unnecessary." "We ASSERT that *death* can never take place without some *important change in organization*. Let our opponents prove the contrary. (2) Further, we *assert*, that in *all cases* in which an organ-

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(1) See Dr. Carpenter's "still closer parallel of the *steam-engine* with the mechanism of *organized structures*," — in his *Principles*, &c. p. 132.

(2) A dog may be killed in an *instant of time* by applying a drop of hydrocyanic acid, or of an alcoholic solution of the extract of nux vomica, to the tongue. A paroxysm of anger, or of joy, will destroy a man with the same instantaneousness. *How* do they

ized body, such as an *egg* or *seed*, exhibits a *vital power* of resisiting the decomposing effect of external agents, *that power is due* to *some* amount of *vital action* going on in it; and it is for our opponents to prove that no such action is going on." "This Dr. Paine has by no means done."

But, what mean the expressions, "*power of resisting*, and *that power* is *due* to some amount of vital *action*"? (*Rev.* p. 390.)

The author will now explain how far he considers "action of some kind" indispensable to *force*, whether physical, chemical, or organic; and, since what he is about to say is no where denied in the "Commentaries," but fully laid down in the work, it will only show, the more forcibly, the reviewer's falsification of his author's doctrines.

1. As to *physical force*. This, in relation to gravitation the author has already explained. He also holds that the force of cohesive attraction *must* constantly operate, or be *in action*, or the particles of matter would fall asunder. And so on. (*Reply*, p. 13.)

2. As to the *force* of *chemical* affinity, that, also, must be in perpetual *action*, or the very elements would disunite. (*Reply*, p. 14.)

3. *Organic force*, even in the egg and seed, must be as much in *constant action*, and in a way analogous to chemical affinity, or the *ternary*, *quaternary*, &c. combinations *peculiar* to organic beings, and which depend on the *organic* force, would separate spontaneously or become the subjects of chemical decomposition. This is even expressed in the author's *definition of life*, so obnoxious to the reviewer. As to the "seed" and "egg," the "Commentaries" say, —

"The history of the seed and egg probably supplies one of the most remarkable illustrations of design that can be found in nature, — especially that of the *former*. They are the ONLY instances where the entire forces of life cease their *ordinary* [appreciable] operation *without becoming extinct*; and, were it not for this interval of *repose*, the species would probably disappear, — since, even if the vital forces *carried out the development* of the seed into the plant, the chances of preservation would be infinite-

disorganize the body? Upon *what* do these causes operate? And, can the reviewer suppose that a fulfilment of his requisition is necessary to the position of the vitalists that in such, (and therefore in all ordinary cases,) death is not the result of "disorganization," but of an *extinction* of the *vital powers*? No agents will afterwards restore animation, because there remains no vital principle upon which the agents may operate. Are not these *facts* as good, at least, as the reviewer's *assertions*?

Again, it is equally assumed that disease consists in disorganization. The whole body is disorganized in idiopathic fever, and locally in inflammations. But, pleurisy, croup, &c. may be mainly cured during the process of venesection; or, a dose of quinine, exhibited just before an expected violent paroxysm of an intermittent, may prevent its occurrence. Do these remedies operate by instantaneously repairing disorganized parts? What is the *modus operandi*, or how does the quinine send abroad its instant influence from the stomach?

ly diminished, — and since, also, such as ceased their operation at the maturity of the seed, are supposed by the proposition to become extinct."

"The rain falls, and there is no check to those actions which had brought the seed to maturity; but the vital stimuli urge them on, and the forces of life pass the ordinary limit of quiescence without a momentary suspension of their actions. These actions, and all their results, are exactly the same as after the seed has been dormant for a thousand years; and their uninterrupted progress in the former instance shows an identity of force before and after germination, and thus connects the principle, on which a renewal of action depends, with that of uninterrupted action. This illustration also shows, that a perfect integrity of the vital forces exists during the state of quiescence, and it is moreover opposed to all analogy, that such forces may be reproduced after they shall have become once EXTINCT.

"Something *like the foregoing* is seen in the *hybernating* animals during their *state of torpor*. The influence of *cold* upon the forces of life nearly extinguishes all vital actions; but that life is undiminished, is sufficiently manifest when the farther and more profound operation of *cold* re-establishes all the phenomena of life in their highest vigour." (*Comm.* vol. i. p. 21—22.)

The reviewer, not content with pirating upon *Dr. Carpenter*, has actually appropriated *as his own* the illustration contained in the last of the foregoing paragraphs, to carry out his conflicting doctrine that "some amount of vital action must exist in the living egg and seed," and by which he was attempting to persuade his reader that Dr. Paine had denied the existence of that perpetual action which obtains with vital affinity during the lowest degrees of life. Thus: —

> "We therefore feel no doubt that the *difference* in the power of *resisting cold* possessed by a living and dead egg is to be accounted for on precisely the same principles as that which may be observed between an *hybernating* and a dead animal." (*Rev.* p. 390.)

Nevertheless, the reviewer may have obtained his idea from Dr. Carpenter, who, in reference to the foregoing subject, says, —

> "The state of *hybernation*, to which many animals are subject, partly resembles the torpor of the seed; still there is never in them a TOTAL SUSPENSION of *vital action*, but only a great diminution." (CARP. *Princip.* p. 142.)

Now, this is so like the author's illustration, that it behooves him to say, that he had not seen Dr. Carpenter's work, (but only the review of it,) till long after the Essay on the "Vital Powers" was printed. As it seems improbable, therefore, that this illustration should have occurred to three writers, it is not unlikely that the reviewer leaned rather upon Dr. Carpenter than the author.

Is it asked, why, after death, and when decomposition is *prevented*, are the *elements still held* together in their *pre-existing* combinations? By cohesive attraction, and, as Tiedemann says, through "*results* of the organic forces which *have been* active in the *living* body." (*Reply*, p. 17.)

The author is now prepared to show how one "philosophical writer," at least, (Dr. Carpenter,) does not allow of *any* "kind of action" in the living egg and seed, under particular circumstances. But, if the reader will compare the following statements by Dr. Carpenter and the reviewer, he will perceive that Dr. C. does not disagree with the reviewer, since the latter only supposes the existence of "some amount of vital action" *when* the egg and seed are exposed to the action of external *stimuli*. Herein, therefore, we have another "philosophical writer" in the reviewer himself, who more than sustains Dr. Paine's "position" that "the forces of life are to a *certain extent* in a passive state when not excited by their appropriate stimuli."

For the foregoing purpose, we require nothing more than the *direct avowal* in the quotation just made from Dr. Carpenter; but, the question being of some moment, we will hear the gentlemen farther; and first, the reviewer, who says:—

> "We ASSERT, that in all cases in which an organized body, such as an egg or seed, exhibits a vital power of *resisting* the decomposing effect of *external agents*, that power is due to *some* amount of *vital action* going on in it, and it is for our opponents to prove that no such action is going on." (*Rev.* p. 390.)

And so Dr. Carpenter:—

> "But the mere CESSATION," says Dr. Carpenter, "whether apparent or REAL of *vital* ACTIONS *does not constitute death*. Their SUSPENSION may result from the want of *stimuli* which are necessary to *excite* the *dormant* properties *to exercise*. Thus, seeds may *preserve their vitality* for periods of indefinite length, if not exposed to those agents which will stimulate them to germination." "It is scarcely correct in such a case to say that the seed is *alive*, since life (in the sense in which most philosophical modern physiologists employ it) is *synonymous* with vital *action*; but it is possessed of *vital properties* or of *vitality*, so long as no destructive changes take place in its organization." (1) (CARP. *Princip.* p. 140.)

Now, it may be difficult to interpret the foregoing passage; but it certainly begins with the declaration that "vital action" is not necessary to life, and this in the reviewer's acceptation of insensible "action" is more than the "Commentaries" have contended for. Will Dr. Carpenter, also, enlighten us farther as to the condition of the egg and seed when they are *neither* "dead" nor "alive," and yet "possessed of *vital properties* or *vitality*?" Immediately after the foregoing extract, Dr. Carpenter quotes an instance in which Raspberry seeds produced "three plants" after laying in the "stomach of a man 30 feet below the surface of the earth probably for 1,600 or 1,700 years," as an illustration of the *intermediate* state of life. But, however Dr. Carpenter may *fix* this matter, and have it in the same paragraph that the living seed, in the absence of stimuli, has *no action* whilst *action* is *necessary* to life, and the living seed is yet *alive*, but *not alive*, it is important to the "Commentaries" where Dr. Carpenter is interested, that it should be shown from his work *itself*, that he makes life to consist in *action*, and yet in the vital properties; since, as just shown, "the seed" may be "possessed of *vital properties* or *vitality*, so long as no destructive changes take place in its organization," and yet have *no action*. Now we shall have adjusted this luminous explanation, by quoting a passage just preceding the last. Thus:—

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(1) Allow any amount of "vital action," it would not show that "life is constituted by the *functions*" and especially since the premises suppose there is no "vital action" when vital stimuli do not operate. The *vital properties*, therefore, constitute life, of which *all* vital action is only a *result*.

> "That there are cases in which a *very feeble* degree of *vital action* is sufficient to *preserve* the *properties* of a structure, will be presently shown. But when THESE ALTOGETHER CEASE, the organism must be secluded from all the external influences which could injuriously affect it, in order that ITS VITALITY may be PRESERVED." (CARP. *Princip.* p. 140.)

Now compare all this with the "Commentaries," Vol. I. p. 22—28;—and, let us also ask how the *vital properties can altogether cease* and yet "*the structure*" have its "*vitality* preserved," especially seeing that it is affirmed in the quotation preceding the last that "*life* is synonymous with *vital action*," and "*vital properties*" with "*vitality*," and more especially as it is clearly affirmed in the first clause of the last quotation, that "*vital action*" is *necessary* "to preserve the properties of a structure;" though this is contradicted in the next clause. The reader will also see another contradiction in the first quotation, where it is said that "the *suspension* of *vital action* may result from the want of *stimuli* which are necessary to *excite* the *dormant* properties to *exercise*,"—it being thus affirmed that *vital actions* are a *consequence* of the *exercise* of the vital properties. This is the nature of the argument held with Dr. Carpenter in the "Commentaries," and which has been so falsified by the reviewer that the subject will be again resumed. In the meantime the author may say that it was partly the object of his reference to Dr. Carpenter, to show that he contradicted himself in the foregoing manner. It is also worth observing, in reference to what will be said of bloodletting, that Dr. Carpenter, like the reviewer, in these quotations, speaks of the *action of stimuli upon the vital properties*, and that these properties are "excited *to exercise*." Now, such contradictions must always abound whenever a writer is at war with nature.

As this "examination" is designed for greater purposes than a mere *reply*, the author will farther avail himself of the foregoing doctrine of Dr. Carpenter, that "vitality" and the "vital properties" may exist for more than a thousand years in a "seed" *without* any vital *action*. Now, if "vitality" or "vital properties" can so exist in any one case, "the functions" or "vital action" do not constitute life in any case; but *life*, (according to the argument with Dr. Carpenter in the "Commentaries,") must, as here allowed, *consist in the vital properties.* It is also true, as affirmed by the reviewer, that, —

> "The *doctrine* which Dr. Carpenter has propounded respecting *vital* properties is *essentially the same* as that upheld by Dr. Prichard, Dr. Fletcher, Mr. Roberton, and other able writers on the same side." (*Rev.* p. 389.)

So that the author has a full admission of his own premises against the assumption by the same writers, that life *consists* in *vital actions* upon which the vital properties are said to *depend.* If philosophy be worth anything it must obtain in the foregoing case; and the conclusion is important in settling the existence, the nature, and the office of *the vital principle*, and its pathological and therapeutical bearings.

The author now approaches other not less extraordinary misrepresentations of fundamental parts of his work, — and these are, 1st, an effort of his reviewer to convey the belief that the author inculcates the absurd doctrines that the organic or any other force can exist independently of organic or inorganic matter; and 2d, that, in employing *vital properties* in the same sense as *vital principle*, the author has *as many vital principles* as *properties.* The following are some of the passages to the foregoing effect. First, as to the *separate* nature of force: —

> "We cannot ourselves," says the reviewer, "conceive of a force as having an existence distinct from the matter which manifests it. The Creator, in giving origin to that which we term matter, by that very act *created the forces* by which different material bodies operate upon one another." (*Rev.* p. 386.)

And so Dr. Carpenter: —

> "The Divine Creator of the Universe 'has, by creating his materials, endued with certain fixed qualities and powers, &c., and made all their subsequent combinations and relations inevitable consequences of this first impression." (CARP. Princip. p. 134.)

Now this very doctrine is elementary in the "Commentaries," and there is sometimes quite a coincidence between the language of the reviewer and his author. Thus, in the very next paragraph following that from which the reviewer has quoted the author's definition of life, is this statement, namely, —

"It cannot, therefore, be said, in an abstract sense, that the forces of life are the primary cause of organization, till it be shown that organization is not the substratum in which the forces are originally *inherent.* He, who *created the powers of life*, associated them with that organization which they were destined to unfold. The *rudiments have been perpetuated in connection with the living forces* since they came from the hands of the Creator, and are the *present source of all animated beings.*" *Comm.* vol. i, p. 18.

> Again, "Dr. Paine," says the reviewer, "maintains that they [the vital properties] *hover about* with a kind of undefined existence, ready to enter organized tissues when ready for their reception," &c. (*Rev.* p. 390.)
>
> "Further, Dr. Paine *tells us* in one breath, that the vital properties or forces are

essentially distinct from organized matter itself, and that they can *go away* and leave this matter in a state of perfect integrity, whilst in the next he informs us that they cannot exist without it. What becomes of them, then, when they leave the organized body to shift for itself? If they have an existence so essentially distinct as to be capable of leaving it, it stands to reason that they must be capable of existing without it, yet Dr. Paine says they cannot. A WRITER WHO CAN SO CONTRADICT HIMSELF SCARCELY NEEDS TO BE EXPOSED BY US." (1) (*Rev.* p. 387.)

But, does not the contradiction consist in a most wilful misrepresentation of the author's doctrine? From what has been hitherto said, it is scarcely necessary to repeat that the author regards the vital principle as being "*distinct*" from the sensible matter of organization in the same sense in which it is defined by Dr. Carpenter, as being generally entertained by vitalists, and as already quoted, (p. 15—17); that is to say, it is *sui generis*, and of course *different* or "distinct" from the matter which it animates. He does not believe, nor has he remotely implied, that the *vital principle* or the *vital properties* can exist independently of organic matter, nor that they "hover about it;" and he believes with all other vitalists that when the *vital principle* "leaves the organized matter," it does not "go away," (nor does he "tell us" so, or *imply* it,) but becomes "*extinct*," and that the "body, in shifting for itself," becomes the subject of chemical agencies. Nor is this all; for it is the principal object of the Essay on the "Vital Powers" to make out the several foregoing propositions. Nay more, these very premises are directed against Dr. Carpenter's theory of life; and here, by the way, the author will have an extract from the "Commentaries," relative to one of the foregoing misrepresentations. Thus:—

"We find the following statement in the last No. of the British and Foreign Medical Review.—'The *dependence* of the *vital properties on the structure*, Dr. Carpenter enforces by a consideration of the nature of death; showing, that when integrity of the organization is maintained *by* the continuance of *its vital action*, (particularly nutrition,) the change of structure consequent on the cessation of the action *necessarily* involves the LOSS of vitality." (*Comm.* vol. i, p. 22. See parallel quotations farther on, and *Reply*, p. 25.)

Now here we have "vital properties" *depending*, 1st, on *vital* action, 2d, on the structure; but when the integrity of organization is "not maintained by vital action, the cessation of vital action necessarily involves the *loss* of *vitality*," or of those "vital properties" which "depend on the structure." "What, then, become of those vital properties" when they are thus "*lost*" and "leave the organized body to shift for itself?" Dr. Carpenter says, with Dr. Paine, that they cannot exist independently of the matter in which they are *inherent*, and yet he says that very matter may *lose* them. But, lest the reviewer, who commended the doctrine, should again *impeach* his own interpretation of Dr. Carpenter's views, they shall be presented directly from the work itself. Thus: —

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(1) There is much in the review that labours to convey the belief that the author has propagated the monstrous absurdity that the vital and other properties of matter can exist in a state independently of that matter. The author will make, in this note, one quotation more from the Review.

"Subtract from *our notion* of matter all those properties by which we characterize it, and what remains? Nothing. On the other hand, we may ask, *if it is possible* for the properties to be separated from the matter itself? Can they have a distinct existence?" And so on. (*Rev.* p. 386.)

> "The term *death*, when applied to *individual* parts, may signify the LOSS of their *peculiar vital properties*, either from some change in their organization, or from the cessation of those ACTIONS by WHICH their STRUCTURE IS MAINTAINED in its due perfection." (CARP. *Princip*. p. 139.)

Now Dr. Paine has not even implied so great an absurdity as that the vital properties may be *lost*. And thus Dr. Carpenter again: —

> "The term *death*, therefore, has more than one signification. It may be used to denote the SEPARATION of THAT BOND OF UNION which so PECULIARLY *unites all the functions* of the living system." (CARP. *Princip*. p. 139. This and the foregoing are the "significations.")

What, then, is this "*bond of union*," but Dr. Paine's "vital principle,"—and where does it "go" when it "separates" from the body? If the reader, also, will connect the two extracts together, which are parts of *one* paragraph, he will see that Dr. Carpenter attributes "vital properties" to his "bond of union" as much as the author does to his "vital principle."

What must we think of those who discourse in the foregoing manner, (and expressions of the same nature abound in Dr. Carpenter's work, and in the review of the "Commentaries,") where they are simultaneously employed in affirming that these "vital properties" are merely the *results* of vital *action*, though at the same time declaring, as will soon be seen, that they were *created in* the very *elements* of matter? Must such contradictions, because exposed in the "Commentaries," (vol. i, pp. 22—28,) be met by the worst fabrications for the purpose of imputing a "contradiction" to one who has probably in no single instance merited the imputation? The author would have cheerfully submitted to any severe analysis of any part or of the entire work, conducted upon the common principles of honour.

Finally, the author repeatedly states his belief that when the body dies the vital principle becomes "extinct." Thus: —

> "This part of our subject naturally leads us to speak, *again*, of the COMPLETE EXTINCTION of the forces of life, when the spiritual leaves the material part, ; and of the impossibility of supposing the extinction of purely material forces."

"They who appear incapable of comprehending the existence of *such a force* as that of *vitality*, or the vital powers, talk as familiarly about *death*, as any others. But *death* of what? Surely not of the *physical* or *chemical* forces; for these are now for the first time in operation, and in furious operation." (*Comm.* vol. i, pp. 50, 98. Also pp. 30, 31, &c. *See Reply*, p. 19.)

The "*onus probandi*," as the reviewer says, "rests with our opponents,—and let them show that" the vital principle does not become extinct;—at least must they show whither it goes when "*lost*" by the organic being.

The next perversion of the author's Essay on the "Vital Powers" represents the author as making as many "vital principles," (and in the author's acceptation of "vital principle,") as he has "vital properties;" or, as the reviewer says, distinct "*entities*."

> "As far as we can understand Dr. Paine," says the reviewer, "all the *vital properties*, as WE SHOULD TERM THEM, appertaining to organized matter, as, for instance, *contractility* and *sensibility*, are to be regarded as *distinct* existences, the combined operation of which produces the phenomena of life. Thus we have not *one* vital principle, but *many*." (*Rev.* p. 387.)

It is, of course, imposssible that the reviewer should have understood any such thing, since he has quoted not only from page 10, where the meaning of "vital principle" and "vital properties" is defined, but from other places where that import is carried out, as it is, indeed, throughout the "Commentaries." "*Vital principle*" is expressly declared to stand for the *only* "active principle," and since *irritability*, *contractility*, *vital affinity*, *sensibility*, and *sympathy*, must belong to the *vital principle*, the author has distinctly specified that he considers them only "elements (endowments or properties) of *that* vital principle." This has been already shown by a quotation, (p. 18.) But, it so happens that the author was, more than once, very circumstantial upon this subject. Thus, again:—

"That we may be FULLY UNDERSTOOD in this place, we will *again* state the creed which we wish to make out. We believe that *the vital principle*, *vital power*, *organic force*, *organic power*, are *one substance*,—and we use these terms synonymously. They refer, with us, to a *universal cause* of animal and vegetable life. We believe, also, that this principle has *various attributes*. Thus, we have *irritability*, *mobility*, or *contractility*, &c. and the *modifications* of each of these in the same or different tissues form our *partial variations*. These properties are also constantly varied in disease, and these *variations* we call *changes* in *kind*. The partial modifications, in their *natural* state, we also call *variations* in *kind*. This is much in conformity with the views of Bichat," &c. (*Comm.* vol. i, p. 81.)

Now the reviewer has quoted nearly the words of the author—

"collective term, referring to the universal cause of animal and vegetable life," (*Rev.* p. 387,) but *conceals* all the author's explanation as rendered here and as before quoted, (p. 18,) and cites another passage from the "Commentaries" (vol. i, p. 29) where the author says, — "The more, therefore, we investigate the subject, the more are we satisfied that life consists in the integrity of the *vital properties* associated with organized matter." Putting these two together, (one from pages 10, and 81, the other from page 29,) he proceeds to extort the conclusion embraced in the foregoing extract. Again, the author says,

"It seems to be admitted, on all hands, that the principle of life, *whatever it be*, is a SIMPLE substance, — that is to say, the presiding forces are not constituted by DISTINCT SPECIES *of substances.* A contrary supposition would be *adverse* to the analogies of nature, and to the astonishing *harmony amongst the* actions of organized matter." (*Comm.* vol. i, p. 82.)

"All we know of the absolute nature of the *organic force* or *vital principle*, resolves it into several distinct properties or forces, some of which may exist independently of others. Thus, we have irritability, mobility, vital affinity, sensibility, sympathy. We infer their *distinctions* from certain well known phenomena. They appear to be INHERENT in the several parts of the system, and not floating about like caloric or the electric fluid. (*Reply*, p. 16. Müller.) They do not even all belong to the different tissues, since some of them are wholly peculiar to the nervous system." (*Comm.* vol. i, p. 80.)

Let us now ask if there be any difference between this mode of considering the *vital principle* and its several endowments or "properties" and that which is universally observed in relation to the mind? Do not the best metaphysicians speak of *distinct* "*properties*" of the mind or soul? Nay more, do not eminent phrenologists go so far as to distribute its "properties" among thirty-six supposed organs of the brain? (*Commentaries*, p. 81.) But, whoever imagined that either the metaphysician or the phrenologist, in thus yielding to the phenomena of the soul, ever meant to imply the existence of as many distinct *souls*, minds, or "entities," as they assume properties or endowments? The author, therefore, puts it to any candid reader whether he has not employed the terms "vital principle" and "vital properties" in exactly the foregoing relation of the mind to its imputed properties. There is nothing, however, as already shown, peculiar to the author in this use of the terms or the ideas which they convey.

The reader will farther see in the following paragraph from the "Commentaries," why the author was so circumstantial in defining the sense in which he intended to employ the several terms already explained. Thus, the Commentaries: —

"Mr. Hunter has also given offence by speaking of the powers of life *in the aggregate*, and calling them *collectively*, as we do, the *vital principle;* when it is obvious from the manner in which he treats of irritability, the principle of motion, sympathy, etc. and the modifications of inflammation, that his conceptions of the *properties of the organic force* were the same as understood by his eminent objectors." (*Comm.* vol. i. p. 94.) Was this a hint to the reviewer? (¹)

Finally, the author will now have a full justification from *Dr. Carpenter* himself. Thus:—

> "It MUST BE ADMITTED that the conditions of VITAL PHENOMENA are not yet determined with sufficient precision, to ENABLE us to REFER ALL OBSERVED FACTS, through the medium of GENERAL LAWS, to simple VITAL PROPERTIES; and there might be no peculiar objection to the use of the term VITAL PRINCIPLE as a CONVENIENT expression *for* the SUM of the UNKNOWN POWERS which are developed by the action of these [vital] properties. But care must be taken not to rest SATISFIED in its use." (CARP. *Princip.* p 131.) So, also, *Reply*, p. 28.

"Unknown *powers* of the *vital principle* developed *by the action* of the *vital properties*"*!* [Compare this, also, with what is said of the creation of "powers" and "forces" by the Creator; *Reply*, p. 9—11.] And here, too, we have a "vital principle" admitted as *perfectly distinct* from "the vital properties" to explain "certain conditions of vital phenomena which cannot be referred to *the simple vital properties*." And yet, on the same page it is said that — "the doctrine of a vital principle is not only *quite unnecessary* to *explain facts*, but is *totally unsupported* by the analogies of nature, and by what we know of the Divine Government in general"! (*Carp.* Princip. p. 131.)

"The veriest hermit in the nation
May yield, God knows, to strong temptation."—*Pope.*

Let us, also, have before us the full extent of this quibbling about the *vital principle* and *vital properties*, and substantiate the inductions of the "Commentaries," by other quotations, that our opponents mean the same thing by "bond of union" and "vital properties" as is maintained by the vitalists, and that they attribute to them all the *reality of existence*, and all the *functions* and *changes* that are claimed by the "Commentaries." This, indeed, has been sufficiently done already; but, since Dr. Carpenter is supposed by his reviewer to have effected the "extinction" of the

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(1) There appears to be a determination to revive this old expedient. Thus, in an article upon Liebig's "Organic Chemistry applied to Agriculture," etc., contained in the present No. of the British Review, there occurs the following remark:

> "Many escape from their difficulties by adopting, with Dr. Prout, the old hypothesis of the existence of *independent vital principles* or *agents* superior to and capable of controlling and directing the agents operating in inorganic matters, on the presence and influence of which the phenomena of organization and life are supposed to depend. The *chemical forces*, *acting* in the system, are subject to this *invisible* cause." (P. 445.)

Now, by no fair construction can it be said that Dr. Prout holds in *any* event, to more than *one* vital principle, and Liebig is very distinct in inferring but *one.* (See *Reply* p. 16.) But, where is the difference between the *distinct* "vital properties," controlling the "chemical forces" of these objectors, and the imputed "vital principles"?

vitalists, (*Comm.* vol. i, pp. 11, *note*, 713,) let us have, in connection with the foregoing quotation, the following extracts, and when we come to the reviewer's remarks on *bloodletting*, our chain of evidence will be complete. Thus, Dr. Carpenter:—

> "The term DEATH, therefore, has more than one signification. It may be used to denote the SEPARATION of THAT BOND OF UNION [*vital principle*] which *so peculiarly* UNITES all the functions of the living system, RENDERING each so dependent on the other, that the cessation of one involves that of all the rest." (CARP. *Princip.* p. 139.)

Now connect the following, from the preceding page, and which is immediately continuous with the extract relative to the "ability of every *uncombined* particle of matter to exhibit vital actions," etc. (*Reply*, *forward*. p. 38.)

> "Experience and observation lead to conclusions not dissimilar. *Organization* and VITAL PROPERTIES are *simultaneously* COMMUNICATED to the *germ* by the structures of its parent; THOSE VITAL PROPERTIES confer upon it THE MEANS of itself assimilating, and THEREBY ORGANIZING and ENDOWING with *vitality*, the materials supplied by the inorganic world." (CARP. *Princip.* p. 138.)

This is exactly the doctrine of Müller as to "*the vital principle*" or "*organic agent*," and who, as we have seen, (*Reply*, p. 15—16.) goes beyond the author and Mr. Hunter in the high attributes which he ascribes to that principle, though he soon afterwards abandons it for the *chemical* and *physical* forces, as set forth in the author's Essay on the Vital Powers. But let us have the parallel from Müller:—

"The *Creative force*," he says, "exists already in the *germ*, and *creates in it* the ESSENTIAL PARTS of the future animal. The germ is *potentially* the *whole* animal. During the development of the germ, the ESSENTIAL parts which constitute the ACTUAL whole are produced." "The ENTIRE VITAL PRINCIPLE of the egg resides in the germinal disk *alone*, and since the external influences which act on the germs of the most different organic beings are the same, we must regard the simple germinal disk, consisting of granular amorphous matter, as the *potential whole* of the future animal, endowed with the ESSENTIAL and SPECIFIC FORCE OF PRINCIPLE of the future being, and CAPABLE OF INCREASING the very small amount of THIS SPECIFIC force and MATTER which it already possesses, BY THE ASSIMILATION OF NEW MATTER." And again, "this *force* exists before the harmonizing parts which are, in fact, FORMED BY IT during the development of the embryo." (1) (*Müller.*)

Now all the foregoing is either, according to Dr. Carpenter, the *work* of the "bond of union" and "vital properties," or, according to Müller, of the "vital principle," and one is as much a *real existence*, an *absolute agent*, a positive "entity," according to the interpretation, as the other,—only, Müller is always consistent in having but *one* agent with certain attributes called "vital properties," whilst Dr. Carpenter has a multitude of *distinct ones*, besides a *general* agent.

(1) Muller's Eleménts of Physiology, p. 23, —1838.

The author now comes to another induction more important than the rest, and which settles, by the showing of these writers, all that is claimed by the vitalists, and in its *fullest* latitude. The "vital properties," says one, and the "vital principle," says the other, are the *real* agents, causes, or *entities*, *by which* "the germ is assimilated, and *by which* materials supplied by the inorganic world are *organized* and *endowed* with *vitality*;" or, according to the other, *which* "*creates* in it the *essential parts*, the *harmonizing parts*, of the future animal, and *increases* the very small amount of the specific force and *matter*, by the *assimilation* of *new* matter."

The rudiments or "essential parts" of the animal, therefore, being thus *effected* by the "vital properties" or by "the vital principle," and being exactly the *same* as all "future" additions, it follows irresistibly from the admitted premises that the "future" growth of the animal, in all its details, depends upon the *same agents*, and that *chemistry* and *physics* have no part or lot in the matter. Here is left no loop-hole for the chemical doctrine. We have our premises from the high places which the author had chosen, as the most acknowledged, to interrogate the proof by which nature or art might stand or fall. And what has just been seen of these leading philosophers is the admitted doctrine in every school of physiology that is entitled to respectful consideration. The demonstration is therefore complete; for, to call in the agency of chemical or physical laws, to accomplish precisely the same results at any "future" stage of the organic being as are admitted to be performed in the development of the "essential parts" of that being by the "vital principle" or the "vital properties" alone, would be such a violation of the plainest rule in philosophy, that it can scarcely be supposed that it will not be admitted that the foregoing question is now settled. The author, therefore, connects this part of his demonstration, especially, with his Essay on the "Vital Powers."

Finally, the three last preceding extracts from Dr. Carpenter show a conflict of fundamental doctrines, not only in respect to the assumed dependence of "organization" upon chemical laws, and the "assertion" that life consists in that organization and vital *actions*, but they prove to us how natural it is for artificial systems to be contradicted by their own authors whenever they begin to reason from the phenomena of nature. The doctrine on which Dr. Carpenter most insists, is well described by Müller in the first of the following sentences; whilst in the others, the foregoing conclusions, which Dr. Carpenter has derived from nature, are set forth in a manner which would, otherwise, lead us to imagine that they had been derived from Müller himself. Thus: --

"Some have believed," says Müller, "that life, — the active phenomena of organized bodies, — is only the *result* of the harmony of the different parts, — of the mutual action, as it were, of the wheels of the machine, — and that death is the consequence of a disturbance of this harmony." "But, the harmonious action of the essential parts of the individual *subsists only* by *the influence* of a *force*, the *operation* of which is extended to all parts of the body, and which does not depend on any single parts. This *force exists before* the harmonizing parts, which are, in fact, *formed by it* during the development of the embryo." "The VITAL FORCE inherent in organic beings ITSELF GENERATES from organic matter the essential organs which CONSTITUTE the WHOLE being. This RATIONAL CREATIVE *force*, (*Reply*, p. 16,) is exerted in every animal strictly in accordance with what the nature of each requires." (*Reply*, p. 33. *Müller*, *ut cit.* p. 23. See *Carpenter*, *ut cit.* p. 138 — 139.)

The author will now take up that part of the review which more specifically concerns Dr. Carpenter's work; and which, therefore, from courtesy, perhaps he should have done first, especially since, from the large space allotted, it is difficult to say whether the review is intended as a fictitious defence of Dr. Carpenter, or as a misrepresentation of Dr. Paine's "Commentaries."

"He quite prevents Dr. Carpenter from complaining, however, of this kind of treatment," says the reviewer, "by the following preliminary apology. 'We notice Dr. Carpenter's opinions without the advantage of reading his work, on account of his high reputation and the encomiums of his able reviewer.' WE RATHER SUSPECT, however, that Dr. Carpenter would have been very willing to forfeit the compliment, for the sake of having his views either fairly represented, or passed altogether without notice." (*Rev.* p. 385.)

The reviewer also pretends that he too has been "misapprehended," and "misrepresented." But, did not the reviewer, who professes to be the same as in the present instance, give us a just interpretation of Dr. Carpenter's "views"? The author has quoted fully and verbally, not only the language of the Review, but of Dr. Carpenter, as stated by his reviewer. He has also carefully revised the whole, and compared it with Dr. Carpenter's work and the review, and is now prepared to say that there is not only no "misrepresentation," but not the slightest "misapprehension." The reader will find the author's extracts and comments in Vol. I. pp. 11, 22—29, 713—714: Vol. II. pp. 117, 119—120, 577. Why the reviewer has thought it expedient to institute this charge, the reader will easily satisfy himself by turning to those pages. The discussion, however, is embraced in Vol. I, p. 22—28, — the other references consisting mainly of extracts. (1)

But let us examine the alleged "misrepresentations" and "misapprehensions," which the reviewer applies as well to himself as to Dr. Carpenter, and therefore makes a joint concern of the defence.

> "We shall," he says, "in our present observations, *identify* our own with them, [Dr. Carpenter's positions.] more especially as Dr. Paine's criticisms are directed to both alike ;" — "he has so strangely misunderstood and misrepresented our own views." (*Rev.* p. 387.)

The first charge relates to Dr. Carpenter, and is as follows :—

> "Dr. Paine accuses Dr. Carpenter of a want of consistency, in applying physical and chemical laws to the explanation of some of the phenomena exhibited by living beings, whilst he attributes others to the *vital* properties of their organisms." (*Rev.* p. 387.)

Now, the only paragraph susceptible of the foregoing construction is the first of page 29, Vol. I, which is of a *general* nature, though connected with the antecedent discussion. It is only the last sentence, however, of that paragraph, which refers specifically to Dr. Carpenter, and in that the *imputed* charge of inconsistency is not involved. (2) But the alleged accusation the author now makes, and will abundantly sustain it by quotations from Dr. Carpenter's work in this "examination."

The author's whole argument with Dr. Carpenter, (*Comm.* vol. i, p. 22—28,) is intended to show the fallaciousness of his hypothesis that life consists in "vital action," and that it must follow from Dr. Carpenter's premises that life is constituted by the "vital properties" of which the *actions* are only *a result* in connection with organized matter ; and that, by his own showing, "he has as much a controlling or presiding agent as Mr. Hunter." Perhaps the author has already shown all that is necessary upon this question ; but, as it is the only one upon which he has imputed contradiction to Dr. Carpenter, and as it is important to the doctrine of life, as defended in the "Commentaries," we will hear Dr. Carpenter again, though at the expense of some repetition : —

> "If the application of the term *life* to some *imaginary* agent which is the immediate cause of vital phenomena, be found *useless* or *injurious*, it may reasonably be inquired what is to be understood by it. If we regard as a *living* being, an organized structure which we observe growing, and moving, and *resisting decay*, it is evidently no improper use of the term to designate by it the *sum of all the actions* performed by such a being, from its first production to its final dissolution." (See Reply, p. 31, *a contrast*.)
>
> "The term *death*, therefore, has more than one signification. It may be used to denote the SEPARATION OF THAT BOND OF UNION [see p. 28] *which so peculiarly unites all the functions* of the living system ; rendering each so dependent on the other, that the *cessation of one involves that of all the rest*.

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(1) In the review of Dr. Carpenter's work (Jan. 1839, p. 168,) he is called "Mr. Carpenter," but in the present, the title of Dr. is prefixed. This is stated to explain the reason why he is called *Mr.* Carpenter in the "Commentaries."

(2) The note at page 117, Vol. II, is a quotation from the *reviewer*, and the language there quoted corresponds with an extract which will be soon given from Dr. Carpenter. (*Reply*, forward, p. 40.)

Or, when applied to *individual* parts, it may signify the *loss* of their *peculiar* vital properties, either from some change in their organization, or from the cessation of *those actions by which* their structure is *maintained* in its due perfection; and their consequent subjection to the laws of matter in general. [See Author's definition of Life, pp. 19, 20.] The first change may be termed *systemic*, or more properly *somatic* death, the second *molecular* death." (CARP. *Princip.* pp. 133, 139.)

See, also, *Reply*, pp. 24, 25, 28, where life is also said to consist in actions or functions. But, let us now have an explicit contradiction of all the foregoing statements that life is "synonymous with vital actions," and that it consists in those actions. Thus:—

"Observation of these ACTIONS leads us to arrange them, as has been already stated, into certain groups termed FUNCTIONS; and *analysis* of the functional changes exhibited by living beings, *terminates* in REFERRING THEM ALL to certain PROPERTIES possessed by their component structures; which properties stand in the same relation to ORGANIZED TISSUES, as do those of *gravitation*, *electricity*, &c. to matter in general. They are CALLED INTO ACTION by STIMULI of various kinds, ADAPTED TO EXCITE each of them to ITS OWN PECULIAR OPERATIONS." (CARP. *Princip.* p. 134.)

This is all that the most "exclusive vitalist" ever contended for,—whether in a physiological, pathological, or therapeutical sense. And now let the reader compare the foregoing and former extracts, pp. 24, 25, 28, with the extract which forms the important part of Dr. Paine's premises, Vol. I, p. 22, as derived from the review of Dr. Carpenter's work, and he will find an exact correspondence: whilst the author's object was to deduce from them the conclusion that life does not consist in *the functions*, but that by Dr. Carpenter's own showing, the *functions* depend upon the "*vital properties*;" and, as Dr. Paine says, that,—"These 'vital properties,' therefore, which have been so much condemned when spoken of under the name of 'vital principle,' or 'organic force,' are exactly *our* vital properties, as they were, also, those of Hunter and Bichat."—This is all the contradiction or "want of consistency" which the author imputed to Dr. Carpenter, and it will have been seen, in the course of this reply, how much farther the author is sustained.

The next "accusation" imputed to the author is equally without a shadow of foundation. Thus:—

"Neither *Dr. Carpenter* nor *ourselves ever* advocated the doctrine that ORGANIZATION could be the result of any chemical or physical laws. On the contrary, Dr. Carpenter most distinctly repudiates any such idea, and shows that these laws can only operate in the preparation of the ORGANIZABLE MATERIALS, which, *until organized, do not exhibit vital properties*." (*Review*, p. 388.)

As the author has affirmed, the foregoing is an absolute misrepresentation of what is said in the "Commentaries" in relation to Dr. Carpenter and his reviewer. It is a mere pretence. Just the *contrary*, indeed, was the object of the author, who endeavoured to show that the premises of both lead directly to an *opposite* conclusion; and, these premises were quoted by the author for the purpose of substantiating, by their authority, his own doctrine of vitality. (1)

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(1) The phrase "organized chemistry," vol. i, p. 27, *line* 23, is *clearly* a *typographical* error for *organic* chemistry. The first is absolute nonsense.

But, we will now have from the reviewer of the "Commentaries" a brief quotation, in which is set forth, very summarily, the radical tenets of the physical school; from which, also, the reader will be able to comprehend how far the reviewer and his associates do "advocate the doctrine that *organization* is the result of chemical or physical laws." Thus:—

> "The doctrine which Dr. Carpenter has propounded respecting *vital properties*, and which is essentially the same as that upheld by *Dr. Pritchard*, *Dr. Fletcher*, *Mr. Roberton*, and *other able writers on the same side*, may be concisely stated as follows:—Certain forms of matter, (especially oxygen, hydrogen, carbon, and nitrogen,) are endowed with properties which *do not manifest themselves* either in these elements when uncombined, or in those combinations of them *which the chemist effects by ordinary means.* But they do manifest themselves when they are united into those peculiar compounds which are known as *organic*, and *when* these compounds have been submitted to the process which is termed *organization.* It is possible that the *first* of these conditions may be imitated by the chemist, but the last can only be effected by a previously existing organism. We ASSERT, then, that the very act of organization causes the materials acted on to exhibit properties quite distinct from those ordinarily termed physical and chemical, which properties cannot be caused to manifest themselves in any other way than by the series of operations just described. We cannot see in what points this doctrine is open to objection. NO ONE CAN SAY that the properties *do not* exist in a DORMANT state because they do not manifest themselves to him." "We argue that they [the *vital* properties] were *as much present in the* ELEMENTS *as any of their other properties, which only exhibit themselves in certain conditions.*" "How do we know that *magnetic properties* may be made to show themselves in *iron*, until we have placed the metal in the necessary relations with a magnet? How, THEN, *can* we expect to find *contractility* or *sensibility* in any combination of *oxygen*, *hydrogen*, *carbon*, and *nitrogen*, until it has been converted into an *organized* TISSUE [blood?] by a previously existing organism? [A very philosophical induction.] And have we any more right to say that 'vital properties,' or 'vital forces,' or A 'vital principle,' have been SUPERADDED in the last case, than to assert that '*magnetic properties*,' or '*magnetic forces*,' or *a* '*magnetic principle*,' have been *superadded* in the former one? A mode of expression which, if it is to mean anything like that which the words import, no enlightened physical philosopher would think himself justified in employing." (*Review*, pp. 389, 390.)

Now, in the first place, the author has proved the falsity of what is here again imputed to him as to the physical properties of matter; but he has an argument to show that the *vital principle* was "*superadded*" to man and animals by the Creator, after He had completed their structure, since which, that principle has been perpetuated in connection with all organic beings. (*Comm.* vol. i, p. 86—106.) This was done by the author to show the distinct *nature* of the *vital principle*, and to contradistinguish it from all "physical forces," which, the author maintains, with greater consistency than the reviewer and Dr. Carpenter, were "created by the Almighty" cotemporaneously with the matter itself. (See *Reply*, p. 9—11.) As to the phrases "magnetic properties," and "magnetic forces," which, by marks of quotation, are imputed to the "Commentaries," they do not occur in the work; nor is there anything to lead to the conclusion that the author has any parallel between the import of *a* magnetic force (of which he supposes but *one*) and the "*forces*" or "*properties* of a vital principle." It is all deception and misrepresentation. (*See Comm.* vol. i, p. 45, where magnetism and vital powers are spoken of in connection.)

Here, as in nearly all the other criticisms and illustrations, the reviewer has apparently acted the verbal plagiarist upon Dr. Carpenter's work. Thus:—

> "It cannot, then, be logically correct," says Dr. Carpenter, "to speak of vital properties as SUPERADDED to organized matter, although an apparent analogy has been drawn from physical science in support of the assumption." "If an analogy exist between the two processes (which can scarcely be denied), it leads us to the belief, that just as the MAGNETIC POWERS [!] ARE DEVELOPED IN IRON, when the metallic mass is placed in a condition *to manifest them*, so the very *act of organization develops vital powers* in the TISSUES which *it constructs*. For NO ONE CAN ASSERT that there does not *exist* IN EVERY UNCOMBINED PARTICLE OF MATTER, which is capable of being assimilated, the ABILITY TO EXHIBIT VITAL ACTIONS, when placed *in the requisite conditions*." (1) (CARP. *Princip.* p. 137.)

Now, 1st. Is it to be allowed that they who deride the doctrine of *vitalism* "in one breath," because the vital principle, like the soul can be only proved by its *almost infinite* phenomena, shall, in the next, assume that *vital* properties exist in the very "*elements*" of matter, where, by their own admission, there is not a *single* phenomenon to sustain the assumption? Besides, what difference is there implied between these assumed *vital* properties as existing in the very "elements of matter," and the *vital principle*, except that the former "exist in a *dormant* state in the elements of matter," and when they become "developed" are ready to take upon themselves some unexplained office?

2d. What is the difference, as it respects "organization" between *that* result and "*organic* compounds"? Are they not one and the same in a vital sense? The chemist *cannot* "effect" the latter "by ordinary means," (p. 37) nor by *any means whatever;* and, the "possibility" which is implied by the reviewer is a mere subterfuge, and Dr. Carpenter shall soon admit it. The doctrine laid down by Müller upon this subject will not be opposed by the *chemical* physiologists,—especially as it *has not been invalidated*. The one or two supposed instances are purely hypothetical, and so allowed by Müller, whose fundamental position is, that,—

"In *mineral* substances the *elements* are *always* combined in a *binary* manner. They are *never* observed to combine three or four together, so as to form a compound in which each element is equally united with all the others. This, however, is *universally* the case in *organic* bodies. Oxygen, hydrogen, carbon, and nitrogen, the same elements which by *binary* combination formed *inorganic* substances, unite together, each with all the others, and form the *peculiar proximate* principles of *organic* beings. These compounds are termed ternary, or quaternary, according to the number of elements composing them." "Although they may be by analysis reduced to their ultimate elements, they cannot be regenerated by any chemical process." "*Another essential distinction* pointed out by Berzelius is, that in *organic* products the combining *proportions* of their elements do not observe a simple arithmetical ratio." (*Müller's Physiol.* pp. 3, 4, — 1838.) This is also the doctrine of the best professional chemists; though, like that at p. 33, it is fatal to all chemical views of life.

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(1) If the inquisitive reader will compare this review with Dr. Carpenter's article on the "Nature and Causes of Vital Actions," (b. I, c. 1.) he will find, throughout, a perfect parallel of statements, "ASSERTIONS," argument, illustration, and even of style and words.

There is *no* distinction, therefore, in the *vital* nature of "organization" and "organic compounds." But, the reviewer and his school, according to the reviewer, maintain that "organic compounds" are the result of *chemical* laws. Therefore, they do maintain that "organization *is* the result of chemical or physical laws." Nevertheless, according to the reviewer, it is only after "*organic* compounds" are converted into what he assumes to be "organization," that the *vital* properties, which are said to be "dormant in the elements of matter," awaken into existence.

3d. Let us, however, have another mode of reaching the foregoing conclusion, that the gentlemen must maintain that "*organization is* the result of chemical or physical laws." The reviewer, in different parts of his article, refers to the nervous power, sensibility, sympathy, irritability, and contractility. Whether he, or the other "able writers on the same side," have any other *vital* properties they have never said. As the matter now stands, we have the *contractility* of hydrogen gas, — the *sensibility* of carbon, — the *sympathy* of quicksilver, — the *irritability* of potassium, — the *nervous power* of phosphorus, and so on; — but especially do all the foregoing vital properties *repose* in "oxygen, hydrogen, carbon, and nitrogen." In all this, as has been shown, Dr. Carpenter agrees. It is the opinion of the present writer, however, that neither the reviewer nor Dr. Carpenter will go so far as to awaken these "dormant vital properties," and assume that the elements unite into "*organic compounds*" in virtue of their "dormant" presence. But "organic compounds" are equivalent to "organization," from which it follows that organization can only result from the "chemical or physical laws."

Notwithstanding, however, the emphatic manner in which it is insisted that the vital properties exist in the *elements* of matter, this doctrine is wholly lost sight of at other times, when both the reviewer and Dr. Carpenter agree with the vitalists that those properties are "*communicated*" by the living organism, and by that alone, — as appears abundantly from our extracts. Farther, also, they are said to *depend* upon vital *action*, and to be "*lost*" when that action ceases. (*Reply*, p. 28, &c.)

4th. Now let us have a series of quotations from Dr. Carpenter illustrative of his views of life, and of the question now before us. They all occur in three consecutive paragraphs, thus:

> "It will be hereafter shown that the ***absorption*** of ***nutritious*** fluid is probably due to the PHYSICAL power of ENDOSMOSE; and the interchange of gaseous ingredients between the air and the blood in the act of respiration, to the transmitting power which all membranes possess. But, the CONTINUANCE of these, and of many more which might be named, is PECULIARLY DEPENDENT on the continuance of OTHER VITAL actions [*none* having been mentioned!], and can only be effected in DEAD matter by PROCESSES which IMITATE these"' "A continued absorption may be produced by a ***physical*** contrivance which ***imitates*** the effects of ***vital*** action, as in the WICK OF A LAMP, which draws up oil to supply the ***combustion above***, but will cease to do so when the ***demand*** no longer exists. IN THE SAME MANNER, the constant ***æration*** of the blood is dependent upon the continuance of the passage of the fluid ***over the respiratory membrane***"'!
>
> "There is another set of changes in which ***vital*** actions would seem yet more intimately concerned, but which still appear to be IMMEDIATELY DEPENDENT UPON THE SAME LAWS as those which REGULATE INORGANIC MATTER [!] These consist in the production, from the alimentary materials, of ORGANIC COMPOUNDS, either such as gum, sugar, albumen, gelatine, &c. which are destined to be STILL FURTHER ORGANIZED. This process must not be confounded with that of ORGANIZATION, since it only prepares the materials upon which that is concerned." (CARP. *Princip.* p. 145. See *Comm.* vol. i, p. 27.)

Now, to understand any part of these conflicting statements, each clause must be taken as an independent whole. Let us, therefore, take the words, "immediately dependent upon the same laws as those which regulate inorganic matter." This gives us what is affirmed by the reviewer, (and what will soon be more direct) that Dr. Carpenter, like himself, supposes that "*organic compounds*" depend upon "*chemical laws*." And yet, in the next following paragraph the whole of this is exploded in the following manner:—

> "We ***cannot yet succeed*** in producing ***artificially*** any ***organic compound***, even of the simplest kind, by directly combining its elements, [mark this] BECAUSE WE CANNOT BRING THEM TOGETHER IN THEIR REQUISITE STATES AND PROPORTIONS; but, there is no reasonable ground for doubt that IF THE ELEMENTS COULD BE SO BROUGHT TOGETHER BY THE HAND OF MAN, the ***result would be the*** SAME as the NATURAL compound, [!] for the AGENCY of VITALITY, as Dr. Prout justly remarks, does not change the PROPERTIES of the elements, but SIMPLY COMBINES THEM IN MODES WHICH WE CANNOT IMITATE"!! (CARP. *Princip.* p. 146, See *Reply* pp. 17, 35, ***note*** 2, 38, and *Comm.* vol. ii, p. 117, ***note.***)

"*Vitality*," then, "*combines the elements*"! Put this with Reply, pp. 33, 39.

5th. But we have, in the review of the "Commentaries," what is equivalent to a distinct affirmation that *organization is* the result of chemical agencies, thus:

> 1. "We shall rest upon the fact, now fully established [!] that, by influences acting on *chemical* principles, one organic product "not an organized tissue" may be converted into another, as distinctly indicating that the *elements of all these products are held together* by no other than CHEMICAL affinities." (*Review*, p. 389.)

Now observe the contradiction in the next sentence.

> 2. "Certain forms of matter, (especially oxygen, hydrogen, carbon, and nitrogen,) are endowed with [vital] properties, which do not manifest themselves either in these elements when uncombined, *or in those combinations* of them *which the chemist* effects by ordinary means." (*Review*, p. 389.)

Now the *whole inorganic kingdom* and the *chemist* are concerned about the same laws in effecting the combinations of elements. By what laws, then, are those combinations formed which the chemist and inorganic kingdom cannot effect? Certainly not the *chemical*, and therefore the reviewer contradicts himself in saying that "the *elements* of all these (*organic*) products are *held together* by no other than *chemical* affinities," since they must be "held together," (at least under ordinary circumstances,) by the affinities through which they were *united*.

Where then, was the use of swelling the "great book" by introducing Dr. Fletcher's, and Dr. Prichard's, views of vitalism, of whose partial exclusion the reviewer complains; especially since the "doctrine propounded by Dr. Carpenter respecting the vital properties is essentially *the same*" as theirs, and "the other able writers on the same side," — whilst Dr. Carpenter gave us, also, the latest revision of the "doctrine?"

Observe, also, the subterfuge in the first of the foregoing extracts, by which the reviewer would have us understand that he means that "organization," or a manifestation of vital properties, consists in "an organized tissue." But, let us settle this construction against him by his own words. Thus: —

"In regard to the main question, we may briefly state our opinion; — that the *vital* properties of the BLOOD, — for with such we have formerly *shown it* TO BE ENDOWED, — are, like its physical properties, CAPABLE OF ALTERATION." (*Review*, p. 393.)

"It is almost impossible to consider it, without admitting that the LIQUOR SANGUINIS IS AS COMPLETELY POSSESSED OF VITALITY AS ANY SOLID TISSUE OF THE BODY." (Carp. *Princip.* p. 287.)

Let us now show farther that Dr. Carpenter agrees fully with the reviewer in the conflicting doctrines announced in the foregoing consecutive sentences marked 1 and 2, — the coincidence reaching even to the words. Thus: —

1. "Reasons have been already given for the belief, that the AFFINITIES which HOLD TOGETHER the elements of ORGANIZED TISSUES, are the SAME AS THOSE which prevail in the INORGANIC world." "Every fresh discovery tends to show that the POWERS IMMEDIATELY CONCERNED, are, like the elements on which they act, THE SAME IN ALL CASES." (Carp. *Princip.* p. 146)

That there should be no doubt about this chemical tenet, Dr. Carpenter has it twice over within six pages; and we shall repeat it on account of another explicit declaration to the same effect which accompanies it. Thus: —

"Reason has been already given for the belief that the AFFINITIES which HOLD TOGETHER the elementary particles of ORGANIZED STRUCTURES, are not different from THOSE concerned in the INORGANIC world; and it has been SHOWN that the tendency to DECOMPOSITION AFTER DEATH bears a VERY CLOSE RELATION with the ACTIVITY OF THE CHANGES which take place in the part DURING LIFE." !!! (Carp. *Princip.* p. 140.)

2. "For no one can ASSERT that there does not exist in EVERY UNCOMBINED PARTICLE of matter which is capable of being assimilated, the ABILITY TO EXHIBIT vital actions, when placed in the REQUISITE CONDITIONS." [Compare this with the requisition of the reviewer, pp. 21, 37.] Again, "There is another set of changes in which VITAL ACTIONS would seem yet more intimately concerned, but which still appear to be IMMEDIATELY DEPENDENT upon the SAME LAWS AS THOSE which regulate INORGANIC matter [!] These consist in the production, from alimentary materials, of ORGANIC COMPOUNDS, either such as gum, sugar, albumen, gelatine, etc., which are destined to be STILL FURTHER organized, or such as urea, etc. This process must not be confounded with that of ORGANIZATION, since it only *prepares* the materials upon which that is concerned." (Carp. *Princip.* pp. 138, 145.)

The author has also other objects in bringing the two foregoing statements into juxtaposition; and first, to indicate the contradiction between what is said of "organized tissues" in No. 1, and of "organization" in No. 2. — Secondly, to show that Dr. Carpenter does affirm in the most direct manner that even "organized tissues" are "the result of chemical or physical laws." (*Reply*, p. 36.) This appears from No. 1. since, if "the affinities of the inorganic world (chemical affinities) hold together the elements of organized tissues," those elements must, of course, have been *united* "together" by the same "affinities." But, what follows in No. 1, from Dr. Carpenter, about "decomposition," &c., is fully conclusive. Nevertheless, we will show, once more, how the spirit of self-contradiction prevails, and how completely the gentlemen acknowledge the existence of "*organization*" of the alimentary matter *as soon as it enters the lacteals*. First, the reviewer, and next, Dr. Carpenter:—

> "The ORGANIZATION and *consequent* VITALIZATION of this substance (albumen) COMMENCES *as soon as*, being taken INTO THE VESSELS, it is admitted into the living system, to which, so long as it remains in the stomach, it is really *external*." (*Review*, p. 397.)

> "With the process of ABSORPTION, strictly so called, the ORGANIZATION of the constituents of the alimentary fluid, and their ENDOWMENT with VITAL PROPERTIES [yet existing in the very *elements* of matter, *Reply* pp. 37, 38, 39,] may be regarded as COMMENCING in animals as well as in plants." (CARP. *Princip*. p. 206.) — Q. E. D.

And yet, are we gravely told by these writers, that "organization" means "organized tissues," and that it "must not be confounded with organic compounds."

In immediate connection with the foregoing subject we have from the reviewer the following very apposite remark:—

> "Now we hold that upon the Newtonian axiom so universally admitted, which forbids the construction of unnecessary hypotheses, it is the part of the philosopher to refuse his assent to any new doctrine of VITAL AFFINITIES, or of the subversion of chemical affinities by *vitality* or any other equally vague speculation." (*Review*, p. 388.)

And so, Dr. Carpenter:—

> "It is a rule in all philosophical speculations not to frame any hypotheses which are unnecessary to account for phenomena." "The chief ground for the assumption of a distinct set of VITAL AFFINITIES, as they have been termed, appears to be," etc. (CARP. *Princip*. pp. 146, 147.)

And so, the "Commentaries":—

"Is there any philosophy," says the Commentaries, "in the unmeaning multiplication of causes [alluding to chemical agencies] especially when the superadded ones will not explain a single phenomenon, or a single result, appertaining to living organized matter." (*Comm*. vol. i, p. 99.)

The author has also quoted in his "Commentaries" the foregoing apothegm from Newton; and, in opposition to the intermingling of the chemical and vital forces for the purpose of expounding the phenomena of organic beings. The spirit of the apothegm is predominant in the Essays on the "Vital Powers," "Animal Heat," "Digestion," and the "Humoral Pathology," and, what is not a little remarkable, the author, for this very reason, is denounced by his reviewer as being exclusive in not mixing up the chemico-physical and vital doctrines of life, and cultivating the no less grotesque inconsistency of a combined pathology of the humoralists and solidists. (Pp. 384, 392, etc.) How far, also, the reviewer himself has observed the foregoing principle, the reader requires no farther illustration.

But the main object of the writer in introducing the above extract, is to call the attention of the reader to that part of it which imputes to the "Commentaries" the doctrine of a "subversion of *chemical* affinities by *vitality*." This, in common with all other vitalists, the author holds to be true only in respect to the vegetable kingdom. Vegetables subsist on inorganic, animals on organic matter. This is fundamental in the "Commentaries." (See particularly, vol. ii, pp. 121—122, 201.) Nor has the author one *novel* idea as to the doctrine of "vital affinities."

6th. It need not be said that the foregoing chemical doctrine of "organization" is the foundation of spontaneous generation, and of that atheistical creed which are considered in the "Commentaries" (vol. ii, p. 123—140.) The reviewer will now comprehend *one* of the reasons, which he professes not to understand, that prompted the author to embrace in his work the "Appendix on Spontaneous Generation." The author will also give the reviewer the benefit of repeating his (the reviewer's) doctrine upon this subject. Thus:—

> "But, we may take this opportunity of stating that our belief in the general proposition, that 'plants or animals of a *high degree of organization* are capable of *producing* from *various parts* of their tissues *beings* corresponding to those of *the inferior orders* of their kingdoms,' has recently been much strengthened by additional evidence." (*Review*, p. 398.) What is the evidence? — See *Comm.* Vol. II, p. 130.

Dr. Carpenter is of the same opinion. Thus:—

> "It appears very difficult, and indeed almost impossible, without some admission of this kind, to account for the production of PARASITIC plants and ANIMALS in the INTERIOR of OTHERS. That their GERMS have been conveyed FROM WITHOUT into the situations where they are developed, must be held as a VERY FORCED SUPPOSITION," etc. (CARP. *Princip.* p. 395.)

Suppose it so; — is not the organization of the "parasite" as absolutely *specific* as that of the more complex animal, — it may be beast, it may be man? Where, then, must this doctrine conduct our philosophers? *Professions*, in such a case, are nothing; and they are nothing when *God* is confounded with *nature*. (See *Reply*, p. 10.) We must look at the inevitable consequence of the PRINCIPLE; whilst Dr. Carpenter and the reviewer have also laid the broad foundation, that all the "vital properties" there are, exist in the "elements of matter," (*Reply*, p. 37—38), and the former goes so far as to say that — "*we may believe that there exists* IN ALL MATTER A TENDENCY TO BECOME ORGANIZED," (*Carp.* Princip. p. 394.) and that the ELEMENTS *may be organized* "*by the hand of man*"*!* (*Reply*, p. 40.) Compare with Tiedemann's doctrine in *Comm.* vol. II, p. 124. There is no difference in principle, however the reviewer and Dr. C. may not subscribe to absolute "spontaneous generation;" and the latter says of the hypothesis "that an elephant, or an oak, (and why not a man?) might be produced by spontaneous or accidental combination of its elements," — that "such a doctrine it is *impossible* to refute, *otherwise than by an appeal to facts*." (*Carp.* Princip. p. 394.) Setting *Revelation* aside as worthless, in the paramount matters of science, may we not bring to bear upon such a question the evidences of the *highest order of Design* which are manifested by the organization of "an elephant or oak," by the final cause of vegetable in its relation to animal life, by the adaptations of each to air, and to various physical conditions of the globe; aye, and by the *instinct* of animals, and *reason* of man, etc.? Such a refutation may not be satisfactory to all; but is it not to him who measures Design by the scale of reason?

We next come to the Essay on the Philosophy of Bloodletting, p. 390 — 392. The reviewer has no other criticism than an admission of his inability to understand what bloodletting has to do with the "vital properties." This is the more remarkable, since the reviewer himself starts with "vital properties" in the very "elements of matter." But, with all this "transcendentalism," his "vital properties" are the merest *necessity* — conceded, either to secure a toleration of the real chemical and physical doctrines, or to render the discussion intelligible. (*Comm.* vol. i. p. 48.) As we have seen, (*Reply*, pp. 24, 35 — 36,) the "vital properties" are declared to *depend* on the *functions* and *actions*, and that "the *elements* of *organized tissues* are *held together* by the *same affinities* as prevail in the *inorganic* world." Whatever the contradiction, therefore, these "vital properties" have, *de facto*, no agency in organic beings, and can only be brought forward for the foregoing reasons. The author has endeavoured to illustrate this principle in his Essay on the "Vital Powers," and again Vol. II, page 12, etc.

The loss of blood, therefore, with the reviewer, operates directly upon the flesh and bones, by emptying their contents, — just as it would if drawn out of a well contrived machine of leather tubes, with an injection-pipe to maintain a circulation. In referring to the author's proposition that the "*direct* impression of loss of blood is upon the *vital properties* of the solids," among other derisions of this doctrine we have the following:—

"Let those esteem it as such who think it of any use to them." "Are we one whit the wiser after all this learned discussion than we were before?" (*Review,* p. 391.)

But, what do the reviewer, and his school, give us in exchange? Is there any thing remarkably luminous in the *mechanical rationale*? The author cannot comprehend how loss of blood should produce its remarkable results but through the direct agency of the vital properties. He also differs from the reviewer in supposing that the organic system is neither a physical nor a chemical apparatus; but composed of elements in such proportions and modes of combination, and such arrangement of these combinations into tissues, which nothing can effect in the world of dead matter. He supposes this to be done through the agency of a real substantial vital principle unknown to the inorganic world; and, he supposes, like all others who make any absolute distinction between a dead and living being, that "material causes can as well operate upon such a principle as upon the mind; for come we must to the latter conclusion before we can reach the soul." (*Comm.* vol. i, p. 84.) He also believes that all vital agents, natural, morbific, or remedial, exert their primary effect upon that principle, or "*vital properties* if it be preferred"; and that, when disease takes place it is in consequence of some modification, generally in kind, of those properties, whilst a corresponding change follows in the functions; and, agreeing thus with Bichat, he has also adopted his language in saying, "that every curative method should have for its *object* the *restoration* of the *altered vital powers* to their natural *type*." (1) It can scarcely be supposed that the reviewer was ignorant that these are the doctrines of Hunter and Bichat; and yet, for some unaccountable reason he has given to the author of the Commentaries the credit of originality,—probably with a view of carrying out more effectually his imputation of speculative opinions. What is peculiar to the author is the application of the principle to the philosophy of bloodletting.

But since the reviewer is so much in accordance with Dr. Carpenter's positions, let us have his authority:—

> "The POWERS which MOVE the blood," says Dr. Carpenter, "may ALTOGETHER result from VITAL operations; yet the MOTION itself is strictly conformable to physical laws." (CARP. *Princip.* p. 133.)

This is exactly the doctrine of the "Commentaries." The "motion" of the blood is purely mechanical; the rest is "altogether vital." But, through what principle is it, but the acknowledged "*vital powers*" that the heart suddenly ceases its action, or nearly so, when syncope follows the loss of six ounces of blood, or from the sight of the lancet, unless it be that the loss of blood and the nervous influence operate directly upon the "*vital* powers" of the heart, by which, it is admitted, the "motion" of the blood is produced? Or is the effect exerted upon the flesh itself? Can you recall the action, should it happen to cease entirely? Why does a drop of prussic acid kill, when whey nourishes? But after all, the reviewer here, also, contradicts himself, and quite agrees with his author; for in speaking of the effects of "unwholesome food," "wretchedness of mind," etc., upon the poor of Europe, he says that these causes "*combine* to *depress* the *vital powers* of those immediately affected." (*Review*, p. 392.) Of course, then, their action is exerted *upon* the "vital powers." The reader will perceive, also, that we have here an admission that the "vital powers" of the *solids* are capable of direct "alteration," as we have already seen of the *blood*. (*Reply*, p. 41.) Nay more; we have also shown that the reviewer "stimulates" the very properties of inorganic matter, and thus makes them the "creators" of those "powers" and "forces" which, he says, were "created" by the Almighty, — and in this, as has also most amply appeared, Dr. Carpenter agrees. (*Reply*, pp. 9—11, 47.) But, as this subject should not be passed lightly over in its connection with the author's doctrine of the physiological effects of loss of blood, he will add a few more extracts from Dr. Carpenter's work, as to the direct action of causes or agents upon the vital properties. We have already seen that Dr. Carpenter and the reviewer affirm that the "properties" of matter, organic and inorganic, *do* and *do not* carry on its changes, "actions," "results;" that is to say, — the "properties" are the causes of the "actions," and the "actions" are the causes of the "properties;" and this, especially, as it regards organic beings. (*Reply*, p. 9—10, etc.) We will now first have an extract corroborative of foregoing ones as to the dependence of the actions, results, etc. upon the "properties," Thus, in deriding the doctrine of the vitalists, Dr. C. holds the following language :—

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(1) Bichat's General Anatomy applied to Physiol. and Medicine, Vol. I, p. 17.

> "Should we not consider it degrading to the dignity of Infinite Wisdom to suppose that at the creation of each world, He had FOUND IT NECESSARY to delegate to a SUBORDINATE the control over its working, — instead of at once impressing upon its elements those simple PROPERTIES, from WHOSE MUTUAL ACTIONS, foreseen and provided for in the laws according to which THEY OPERATE, *all the varieties of change* which it was His intention to produce, should NECESSARILY result!" (CARP. *Princip.* p. 132. — (See *Reply*, p. 10; and *Comm.* vol. i, pp. 10, 25.)

Here, then, we have another of those affirmations in which *every thing* is made to devolve upon the "properties" of matter, and by which the author is farther sustained in his argument with Dr. Carpenter, and in which he drew from Dr. C. though in opposition to Dr. C. himself, the conclusion that life is essentially constituted by the "vital properties," and not by the "functions." (See *Comm.* vol. i, p. 22—28.)

Having now *once more* got the *vital properties* as the real agents, and causes of all vital actions, let us see again, how far Dr. Carpenter will sustain the author as to the capability of those agents being *acted upon.* This we find in the third sentence from the foregoing extract. Thus:—

"For, if we come to inquire into the FUNCTION of any single organ, or, in other words, into the nature of the changes produced by it, we find that IT may be REFERRED TO THE PROPERTY of the structure, manifested or CALLED INTO ACTION by a STIMULUS of some kind, to which it is EXPRESSLY FITTED TO RESPOND. THIS is evidently the case even in the INORGANIC world"! (CARP. *Princip.* p. 132.)

Now, as to the *organic* world, the most exclusive vitalists never went farther than Dr. Carpenter does in the foregoing extract; and, in respect to the "inorganic," no vitalist, (certainly not the author, *Reply*, p. 18—19,) ever supposed that the properties of dead matter could be "*stimulated*" like the vital properties. It will be seen, also, that the author has here had it conceded to him, by one of the most uncompromising adversaries of "vitalism," more than he requires to sustain his fundamental doctrine as to the physiological effects of the loss of blood, and of all remedial agents, as well as his corresponding induction that the passions, and the nervous influence, produce their direct impression upon the vital properties. And, just so we have seen of the reviewer. Take the affirmative side from either, and they go even beyond the author in what the reviewer denominates "transcendental vitalism." We wish to make this subject very clear, and to show that Dr. Carpenter has as much upon *one side* as on the *other*. Thus:—

"The very STIMULI," he says, "which would operate in EXCITING the VITAL PROPERTIES, as long as the organism retains them, have the effect of facilitating its decay when death has taken place." (CARP. *Princip.* p. 143.)

Precisely what is involved in the author's *definition of life.* (See *Reply*, pp. 8, 20, &c.) And how exactly conformable, also, to the author's doctrine is the following:—

"Every tissue possesses VITAL PROPERTIES PECULIARLY ITS OWN, BESIDES that which has been spoken of AS COMMON to all; and EACH PROPERTY of each organ has STIMULI APPROPRIATE to itself." (CARP. *Princip.* p. 148.)

In coincidence with the author, Dr. Carpenter's "BOND OF UNION," (*Reply*, pp. 28, 31—32,) is the foregoing "common" principle, which is equivalent to the author's "vital principle;" and Dr. Carpenter's "vital properties," which appertain to the "common" principle in this instance, but held distinct at another time, (*Reply*, pp. 28, 31—32,) are the author's "elements or endowments of the vital principle." (*Reply*, pp. 18, 29—30.) Again:—

"Observation of these ACTIONS leads us to arrange them, as has been already stated, into certain groups termed FUNCTIONS; and *analysis* of the functional changes exhibited by living beings, *terminates* in REFERRING THEM ALL to certain PROPERTIES possessed by their component structures; which properties stand in the same relation to ORGANIZED TISSUES, as do those of *gravitation*, *electricity*, &c. to matter in general." "They are CALLED into ACTION by STIMULI of various kinds, ADAPTED TO EXCITE each of them to ITS OWN PECULIAR OPERATIONS." (CARP. *Princip.* p. 134.)

And yet again:

"Their suspension [*vital actions*] may result from the want of the STIMULI which are NECESSARY to EXCITE the DORMANT properties to EXERCISE." (CARP. *Princip.* p. 140.) Real, EXERCISING, agents.

But once more, by way of "illustration:" —

"Besides the vital stimuli, the influence of which is *necessary* for the EXCITEMENT and RENOVATION of the PROPERTIES of living beings, there are others which may produce an influence of a different kind, by calling into play the *animal* functions." "The mode in which they produce their effects is, however, so *analogous* to that in which VITAL STIMULI OPERATE, that from their evident (because only occasional) action, a good ILLUSTRATION may be drawn of the more constant (and therefore less observed) influence of the latter. Thus, *a pinch of snuff* applied to the membrane lining the nostrils, *immediately* excites an increase in its secretions," &c. (CARP. *Princip.* p. 150.)

And now a remark from the review, which also goes to confirm the accuracy of the author's induction, that however his opponents may hypothetically assign organic processes to the forces of dead matter, the moment they are crowded in discussion, or begin to reason from the phenomena of nature, they suffer the whole work to devolve upon *a vital principle.*

"Whatever difference of opinion," says the reviewer, "there may be as to those changes of composition which are concerned in the formation of *organizable materials* and the *products of secretion*, [see *Reply*, p. 38—42,] there is NONE WHATEVER as to the fact of these materials, WHEN ORGANIZED, being ENDOWED [yet *existing in the* ELEMENTS of matter, pp. 37, 38,] with PROPERTIES ENTIRELY DISTINCT from any which CAN BE TRACED *in inorganic matter*"! — (*Rev.* p. 389. See *Reply*, pp. 11—12, 24—25, 28—41.)

Apply the foregoing, and what has been formerly shown, (*Reply*, pp. 24, &c.) to the author's doctrine of the physiological effects of loss of blood, and of other remedial, and morbific, agents, and there is not the slightest discrepancy. The only difference is, the author adheres uniformly to the principle, whilst Dr. Carpenter and the reviewer sometimes sustain it, and at other times declare it all nonsense. When Dr. Carpenter, however, comes to his chapter on "VITAL STIMULI," (p. 149—162,) he is pretty much a "vitalist" throughout, — and, how could he be otherwise, and be intelligible, on such a subject? (See *Comm.* vol. i, pp. 26, 48 —49.)

An artful attempt is made by the reviewer to impress the reader with the belief that the author attributes to the vital properties a species of intelligence. Thus:

"The 'alarm felt by the extreme *vessels* and *capillaries*,' according to Dr. Paine, must be tolerably like the 'sense of danger' which Dr. Macartney attributes to them." (*Rev.* p. 391.)

This is the only instance in the "Commentaries" from which the foregoing construction could be extorted; and the reviewer has seized upon it, notwithstanding his author has repeatedly and strongly objected to the doctrine of Van Helmont, of Stahl, and even of Hunter, which imputes more or less intelligence or instinct to the vital principle. (*Comm.* vol. i, pp. 11, 29, 92, 94;

vol. ii, pp. 169, 174.) The very obvious motive for employing the word "alarm," was to represent figuratively, and therefore more forcibly, the susceptibility of the extreme vessels to the loss of blood. The author will now do, what the reviewer has not done, refer the reader to the page of the work, Vol. I, p. 126, where he will readily see the nature of the injustice which is attempted by the reviewer. In immediate connection with this, also, the reviewer represents the philosophical part of the Essay on Bloodletting as being comprehended in *four* lines which he quotes from Dr. Macartney on Inflammation, — such being the general style of this review. In the first place, however, there is no parallel between the objects of the two writers, and Dr. Macartney's *four* lines relate merely to an *ultimate* effect of bloodletting in a particular disease. But the reader shall be the judge both of the reviewer's honesty and of the author's ground of complaint. The following are the lines, introduced by a coarse remark of which the author presents only the least exceptionable part: —

> "Dr. Paine's general theory," says the reviewer, "may be concisely stated in the following proposition set forth by the latter, [Dr. Macartney.] 'The intention of drawing blood from the system is to produce that kind of impression which is followed by a weaker action of the heart, and a more contracted state of all the smaller arteries in the body, and by that means inflamed parts are included in the general condition which precludes the possibility of inflammation.'" (*Review*, p. 391.)

This is no more "Dr. Paine's general theory" than the most mechanical doctrine which he has endeavoured to controvert. It is the object of Dr Paine's Essay to treat of the *physiological* effects of loss of blood, and to *trace* them from their beginning to their consummation in syncope, — to consider them under *various pathological* conditions, — to indicate the *agency* of the *vital properties*, and how far the *nervous influence* is instrumental in the effects, — to show the *physiological distinctions* between *general bloodletting*, *cupping*, and *leeching*, — to *apply* all this, and much more, under a variety of *practical* aspects, &c.

The affirmation which follows next, that "Dr. Paine tells us that 'a victory is obtained over the disease,' (inflammation) even before the blood is expelled from the vessels," is neither quoted in the author's language, nor does it convey the author's obvious meaning. The reference being suppressed, the reader will find the remark at Vol. I, p. 127. The author may also say, that it is a doctrine of the "Commentaries," that art *never cures*. All that is accomplished by our remedial agents is to establish a change in the diseased properties of life which shall enable nature to take on her general tendency to a restorative process. Our remedies are to be regarded in the same relative sense, and as operating upon the same principles, as morbific agents: only, in the former instance, a morbid condition is substituted which is more favourable to the recuperative process of nature. Hence the importance of making, as speedily as we may, the right impression, — neither too much nor too little, (*tuto, cito, et jucunde,*) — and then fall back upon that system of *watching* which should mainly obtain in all the strictly self-limited diseases, unless complicated with inflammations of important organs. (See *Comm.* vol. i, p. 641—661; vol. ii, pp. 174, 664, 670, &c.)

The reviewer next adverts to the author's strictures upon Dr. Hall's unqualified rules in bloodletting, and observes that, — "*Dr. Hall is as ready as any one to admit that no principle of treatment is without its exceptions, and that this is the case with regard to the rules which he lays down.*" (*Rev.* p. 391.) But, this is only the reviewer's opinion; but being so, is not the principle, in common justice, equally applicable to Dr. Paine? Why, however, did not Dr. Hall state more of the important exceptions in respect to a remedy of so much moment? This brings us to that most flagrant falsification, that the author of the "Commentaries" has not indicated the injurious effects which may result from bloodletting. But, let us hear the reviewer: —

> "Believing," he says, "that, *whatever* may be Dr. Paine's qualifications *as a philosopher*, he is a man of CORRECT and EXTENSIVE observation, (on subjects, at least, on which his prejudices allow him to use his sight and his sense,) we can only justify the very free and ALL BUT UNIVERSAL recourse which he advises to the use of the lancet, by supposing that his countrymen, young and old, have much more stamina than the original stock, — beef-eating and plethoric as it has been usually accounted. (*Rev.* p. 391.)

It happens, however, that no little part of the "Commentaries" is especially concerned in illustrating the pernicious and destructive consequences which may ensue from loss of blood under many conditions of disease, whilst the author is simultaneously employed in attempting to point out *those* conditions, as well as the *physiological* nature of the injurious influences. The author, indeed, has devoted two long sections, (Vol. I, pp. 239—274, and 342—362,) to an exposition of its injurious effects by analyzing the principles of its operation in such instances. The author, also, more than once reminded the reader that: —

"In all our remarks upon the pathology and treatment of disease, we have reference to those *early stages* of its existence, when the powers and actions of life are fairly within the province of art." (*Comm.* vol. ii, p. 531. See, also, various precautionary and explanatory suggestions, vol. i, pp. 213—214, 222. 224, 192 259, 306—308, 621, 149, 232, 216, 235—237, 273, 165, 185—209, 621, 646—661, 275—277, 152—155; Vol. II, pp. 273, 503—505, 527—536. 541, 742.)

It is, also, in inflammations, and in idiopathic fever complicated with local inflammations or congestions, alone, that the author has advised the remedy; whilst, in a large class of affections which he excludes from those denominations, he holds that bloodletting is more or less injurious. Such is true of the simple *nervous*, as they are called, — of the various conditions of indigestion when not attended by inflammation, — of all the strictly self-limited diseases, as smallpox, measles, scarlatina, whooping-cough, mumps, &c., unless demanded by some supervening inflammation of internal organs, &c. What measure of indignation, therefore, may not the author mete out to one who thus, from page to page, from line to line, brandishes falsehood in the very face of fact?

Then follows, in relation to the reviewer's most unfounded charge as to indiscriminate bloodletting, another ironical admission of its possible propriety among the author's countrymen:

> "We are inclined to think," says the reviewer, "that he especially needs enlightenment as to the prevailing constitution of the poor of our large cities, where deficient and unwholesome food, &c. depravity and wretchedness of mind, and all the other evils of squalid poverty, which can scarcely exist in anything like the same extent or degree in any part of the United States, not only combine to *depress* the *vital powers* of those immediately affected, &c. (*Rev.* p. 392.)

The foregoing are exactly among the instances which the author has most carefully, and in various places, (see the foregoing references,) exempted from the general principles which he has set forth in relation to bloodletting. Nay more, the author has protested against taking such subjects for the purposes of any great pathological or therapeutical principles. (*Comm.* vol. i, p. 302—303, and the Essay on the Writings of M. Louis.) True, the author has objected to the "bark and wine" treatment, even of that denomination of patients, *as a general plan*, in *those diseases where* the *antiphlogistic* treatment would be required by *better* constitutions. But, in all his disquisitions upon this question, the author, as he repeatedly avows, has a constant reference to "the early stages of disease." As to the imputed want of "enlightenment" about the poor of European cities, and of European practice, whether it have been made with justice, the reader will be able to decide by referring to Vol. I, p. 279—287, 290, 296—308, 312—330, 332—344; Vol. II, p. 667—676, 631—633, and Essay on M. Louis' Writings. The reviewer, therefore, leaves untouched the comments upon that stimulant treatment of fevers and inflammations among better classes of society which is not very uncommon in European cities, and of which the author was distinctly and mainly speaking, when urging the importance of bloodletting.

We have now reached the Essay on the Humoral Pathology, (*Rev.* p. 392.) Here, as in all the Essays which immediately concern the vital powers, there is no common ground between the reviewer and his author. One is a *chemico-mechanical physiologist* and *humoralist*, the other a *vitalist* and *solidist*. Objections only of a general nature are made, the sum of which is embraced in the statement that this Essay is "a manifesto of 300 closely printed pages." The reviewer sees a great deal of corrupted blood as the primary cause of disease, whilst his author maintains a primary derangement of the solids, and that the alterations of the blood are consecutive. The reviewer, however, has one remark in this place, (already quoted for another purpose, p. 41,) which induces the hope that he may yet give in his adhesion to *vitalism*. It is this: —

> "In regard to the main question," he says, "we may briefly state our opinion, — that the *vital properties* of the *blood* are, like its physical properties, *capable* of ALTERATION by various causes." (*Rev.* p. 393.)

What, then, does the reviewer mean by this supposed "*alteration* of the vital properties"? Of course, exactly what he condemns in the vitalists. It is not the blood itself which is first altered, but its "*vital properties*." Then they are *something* susceptible of "alteration." But, in order to be "altered," something must act upon those "vital properties of the blood." And yet the reviewer professes not to comprehend this language when employed by the vitalists in relation even to the solids; nor do the latter allow that "the vital properties of the blood are altered" by any other "causes" than by the living solids upon which its formation and integrity depend. And, that such is truly the meaning of the reviewer, is amply attested by our quotations at pp. 10, 12, 31, 32, 46—48.

The author will now exhibit one of the minor instances of perversion. The reviewer quotes "three consecutive sentences," which the author will present according to their arrangement in his work. Thus: —

> "These considerations will show us that *the blood is neither a primary cause of disease in the solids, in virtue of its own morbid condition, nor can it be an aggravating cause of disease when altered in its character by the morbid action of the solids.*
>
> "None will deny what is affirmed by M. Andral, that every morbid change in the action of the solids is probably followed by a change in the blood; whilst we fully agree with him, that any *primary* alteration of the blood, of a morbid nature, must, with greater certainty, produce disease of the solids. The latter proposition is the basis of humoralism." (*Ital.* as in *Comm.* vol. i, p. 646. — *Rev.* p. 392.)

The reviewer then assumes that the author's proposition in the first "sentence" is "contradictory" of what he makes a third "consecutive sentence," beginning with the word "whilst;"—the first proposition being an induction from the author's premises, the last from those of the humoralists, which had been stated at great length. But, the reviewer suppresses the next following sentences, which would have cleared up the "difficulty" which he had created. Thus:—

"There is a *specious* parellelism about the foregoing propositions of which humoralism has taken no little advantage. It supposes that the blood and the solids sustain, reciprocally, the same relation to each other; when, in truth, the distinction is about as great as between an agent and the object acted upon. There is this difference, however. In the present case, in their natural state, the blood is the object, whilst it contributes to the support of the agent; but WERE the blood to become *primarily* diseased, it would *then assume the same relation* to the solids as *any other morbific cause*, and even more so on account of the foregoing principle, or in other words, its constituting the *pabulum vitæ*." (*Comm.* vol. i, p. 646.)

The main object of the Essay is to show that the blood is never *primarily* diseased, in the *humoral sense;* but, to prevent all misapprehension, the author carefully contradistinguished those natural changes of the blood which arise from deficient or other imperfect *nutriment*, (though not as a *primary* cause of disease in the solids, *Comm.* vol. i, pp. 609—626, 691—698,) and endeavoured to show, at no little extent, that the *ordinary morbific* agents can have no *primary* effect upon the blood. And, although the reviewer was quoting, in the present instance, from what he found an unanswerable argument to the foregoing effect, and professes to have read the Essay with "the best attention," he goes on to remark immediately after the quotation just made, that,—

"The first of these sentences appears to us so far contradictory of the third, that we can only get out of the difficulty by supposing it to be our author's meaning that no *primary morbid alteration can* take place in the blood,—an opinion so completely opposed to all that sound physiology and pathology teach us, that *we scarcely venture to attribute it to him*." (*Rev.* p. 392.)

What is the object of the reviewer's doubt as conveyed in the last words of the foregoing quotation? Does not the reviewer clearly endeavour to imply, that the author *has not* investigated the question, when it is the main object of the Essay to prove two propositions,—namely, that if "a *primary* disease of the blood" *produce disease* of the solids, there can be no recovery; and, secondly, therefore, in all curable diseases the morbid condition must begin in the solids; otherwise, the blood being naturally *dependent* on the solids, but now becoming more and more diseased, and, therefore, *more morbific*, would acquire an *irretrievable ascenden-*

cy over the solids. "*Nothing* can make *healthy* blood, but a *healthy action* of the solids." (*Comm.* vol. i, p. 641.)

The researches and doctrines of Haller, Prochaska, Whytt, Philip, Reid, Hall, Müller, and Valentin, as to the "*vis insita*" or *organic force*, and the "*vis nervea*" with its wonderful attributes, being fully before the world, the author has little apprehension for the fate of his Essays where vitalism and solidism are concerned; nor for his principal doctrine that all organic processes are dependent on the organic force, and are only influenced incidentally by the nervous power.

Take also the following from the review of Dr. Prout's recent edition of his work on the "Nature and Treatment of Stomach and Urinary Diseases, 1840," contained in the *present number* of the British Review; and let the author of the "Commentaries" ask whether the spirit of the remarks does not sustain the whole foundation of his Essays relative to vitalism and solidism? Thus:—

"Our general estimate of Dr. Prout's treatise may be gathered from the analysis just concluded, and the strictures scattered through it. We acknowledge and have pride in bearing testimony to the high qualifications of our countryman in the branch of PATHOLOGICAL inquiry BASED UPON CHEMICAL FACTS. We recognise the comprehensive sagacity of his speculations, and have respect for the patient zeal with which he has toiled TO ERECT UPON THESE A STABLE SYSTEM. But we fear the time for SUCH SYSTEMATIZING has not yet come; and, although all *speculations* on the subject are SEDUCTIVE in themselves, and doubly so when emanating from an individual of Dr. Prout's eminent skill in the department of chemical physiology, it *cannot*, we think, *be denied*, that in the existing *unformed* and *vacillating state of* ORGANIC CHEMISTRY, (*Reply*, pp. 38—39, 40,) THEY SIN ESSENTIALLY in being established on A MOST UNSOUND BASIS. Nor can we avoid entertaining some *solicitude as to the results of their propagation, which to us appears likely to betray minds of* INFERIOR ORDER *into mere extravagances.* For these, however, Dr. Prout is not *fairly answerable;* and SHOULD his doctrines—when the FRAIL EMBRYO science on which they are *based* has reached healthy maturity—be recognised as true, he must almost TAKE RANK with those highest intelligences, whose energy has OUTRUN the scienitfic apprehension of their times. But, meanwhile, Dr. Prout has *neither* done his doctrine, himself, nor his readers justice, in not explicitly stating the FOUNDATION for and manner of verifying (so far as he is acquainted with these himself) his PRESUMED results. THE DAY HAS PASSED NEVER TO RETURN, WHEN THE AUTHORITY OF NAME COULD AT WILL SUPPLY THE PLACE OF DEMONSTRATION. Should

Dr. Prout intend favouring the public with further volumes, he would do well to ponder on these admirable remarks of a natural philosopher, of an eminence not inferior to his own :—'It is not sufficient to say that we have *seen* such a thing. It is saying nothing, if, at the same time, we do not indicate *how* we have seen it; if we do not give our readers the opportunity of judging of the *manner* in which the facts which we record have been observed.'" (1) (*Brit. and For. Med. Rev. April*, 1841, p. 363.—See *Reply*, p. 4.)

May it not also be, that an observation of the principle enjoined in the last clause of the foregoing quotation is greatly at the foundation of the other reviewer's complaint against a "great book?" And, what a contrast between the courtesy, the honesty, the doc-

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(1) We have had exemplifications from Dr. Carpenter of the influence of the chemical and physical hypotheses of life upon the philosophy of vital actions; and in the "Commentaries" they are sufficiently numerous from Dr. Prout, Müller, and other chemical physiologists. Let us now place in apposition a few extracts from Liebig's latest work, who, like Prout and Müller, has his controlling "vital principle," and like them justly enjoys "a European reputation." (See *Rev.* p. 436.)

"It is impossible," says Liebig, "to mistake the *modus operandi* of putrefied sausages, or muscle, urine, cheese, cerebral substance, and other matters, *in a state of putrefaction*." "It is obvious that they COMMUNICATE THEIR OWN STATE OF PUTREFACTION TO THE SOUND BLOOD, *from which they were produced*, EXACTLY IN THE SAME MANNER AS GLUTEN in a state of decay or putrefaction CAUSES A SIMILAR TRANSFORMATION IN A SOLUTION OF SUGAR."!

"The MODE OF ACTION of a MORBID VIRUS exhibits such a STRONG SIMILARITY TO THE ACTION OF YEAST upon liquids containing sugar and gluten, that the two processes have been long since COMPARED to one another, although merely for the purpose of illustration. [They have often been represented as *identical*.] But, WHEN THE PHENOMENA attending the action of each respectively are considered MORE CLOSELY, it will IN REALITY be seen that their influence DEPENDS UPON THE SAME CAUSE." "*Ordinary yeast*, and the virus *of human smallpox*, effect a violent tumultuous transformation, the former in vegetable juices, the latter in blood."! "The action of the virus of cow-pox is analogous to that of the low yeast.[!] It communicates *its own state of decomposition* to A MATTER in the blood, and from a SECOND MATTER is itself regenerated."! "The SUSCEPTIBILITY of infection by the virus of human smallpox MUST CEASE after vaccination, FOR the SUBSTANCE to the presence of which this susceptibility is owing HAS BEEN REMOVED from the body by a peculiar process of DECOMPOSITION artificially excited."! "COLD MEAT is always in a state of decomposition; it is possible that THIS STATE may be communicated TO THE SYSTEM of a feeble individual, and may be one of the SOURCES OF CONSUMPTION."!

"Analogy, that fertile source of error, has unfortunately led to the very UNAPT COMPARISON of the VITAL functions of plants with those of animals."! "A COTTON WICK, INCLOSED IN A LAMP, which contains a liquid saturated with carbonic acid, ACTS EXACTLY IN THE SAME MANNER AS A LIVING PLANT in the night.[!] Water and carbonic acid are sucked up by capillary attraction, and both EVAPORATE from the exterior part of the wick."! "ALL SUBSTANCES in solution in a soil are absorbed by the ROOTS of plants, EXACTLY as a SPONGE imbibes a liquid, and ALL that it contains WITHOUT SELECTION."! (*Liebig's* Organic Chemistry, applied to Agriculture and Physiology, pp. 26, 33, 92, 349, 350, 370, 372, 373.)

Compare the last with quotations from Liebig at p. 16. There is much in trines, the mind, of the reviewers of Dr. Prout and of Dr. Paine, and in articles which stand side by side!

As appropriate to the present subject, the author will now transcribe, in a *note*, from a lecture by Professor *John P. Harrison*, M.D., of the Cincinnati Medical College, (and which has this day, for the first time, fallen into his hands,) one of those luminous passages which unfolds that deep thought and research which the present writer has stated in his "Commentaries" as characterizing his brethren of the West, where nature is studied in her simplicity, and where so much evidence has been given, that the practice of medicine is pursued with as sound a reference to correct pathology as in any other part of the world. (1)

Liebig of the foregoing nature; and, as to the humoral pathology, he goes far beyond the ancient excesses. (See *Comm.* vol. i, p. 417—420.) Nevertheless, what has been now quoted is a fair exemplification of the fruitful results of chemistry and physics in their relation to the "present state of medical science." The philosophy of Liebig as to vegetable functions is literally carried out by Dr. Carpenter (*Reply*, p. 40) and by Müller (*Comm.* vol. i, p. 565, 683—689) to the animal kingdom. The *agricultural* part of Liebig's work, however, so far surpasses anything hitherto given to the public, we would not have quoted his *physiology* and *pathology*, had it not been for the sake of our cause, and for many passages like the following:—

"*Animal* and *vegetable physiologists*," he says, "institute experiments without being acquainted with the circumstances necessary for the continuance of life,—with the qualities and proper nutriment of the *animal* or plant on which they operate, (*Comm.* vol. i, p. 697—698,) or with the nature and chemical constitution of its organs. These experiments are considered BY THEM as convincing proofs, whilst they are fitted only to AWAKEN PITY." (*Liebig*, ut cit. p. 42.)

Finally, "We see, therefore, that this MYSTERIOUS (VITAL) PRINCIPLE has many relations in common with CHEMICAL FORCES, and that the latter can indeed REPLACE IT"! (*Liebig*, p. 58.)

But, Liebig's "*vital principle*" is as much an "*entity*" as the author's, and his "chemical forces" must, according to the foregoing, be equally so as his vital principle, and, therefore, far exceeding the author's construction. (See *Reply*, pp. 16—17, 9, 13—14, 18—20, 26, 28, 30, 33, 37.)

(1) "FALSE THEORY.—It is a well established fact, that some of the most erroneous theories in medicine have originated from men, who professed to despise theory. And those men who are most addicted to expressions of contempt against reasoning in medicine, are the most disposed to indulge in crude speculation. And the cause of this is obvious. It springs from too exclusive reliance on their own individual experience, apart and independent of that accumulated wisdom of ages to be found in books. 'To think is to theorize,' is a proposition not to be successfully controverted. So deep in man's intellectual nature is laid that disposition to account for phenomena, on which all philosophy is built, that he cannot be induced to forego the gratification it affords, under any condition of his being. The employment of reducing truth to its element, is one of the most gratifying and useful occupations of the mind.

"The luxuriant growth of our science, from the multitude of facts which have been collected by its assiduous cultivators, demands that comprehensive and accurate principles should be deduced from its many and insulated particulars. It is the object of correct theory to reduce the multifarious appearances of disease to simplicity and order.

The reviewer, farther on, recurs to the Essay on the Humoral Pathology, — not satisfied with the representation he had made of that Essay in its appropriate place. We have, therefore, the farther misstatement, that, —

> "According to our author, although blood may be altered from its healthy condition, IT NEVER CAN BE, STRICTLY SPEAKING, DISEASED; that is, it must always bear a *constant* relation to the solid tissues. This he endeavours to establish in his Essay on Humoral Pathology; to which it seems to us quite sufficient to reply that, in all cases of *local* disease, the blood must bear a *different* relation to the healthy and diseased solids respectively." (*Rev.* p. 398.)

In the present state of our science, there is an imperative necessity for the exercise of an inquisitive and powerful reason, to remodel and arrange the facts that are already ascertained, and to trace up the analogies, which run through diseases, to some general principles. Thus we shall be enabled to make a discriminating survey of the miscellaneous variety of particulars which are placed beneath our observation. To frame a correct theory, we must keep in mind the remark of a distinguished medical teacher, — 'Medicina neque agit in cadaver, neque repugnante natura aliquid proficit,' — that medicine will neither act on a dead body, nor will it act on a living body in a way contrary to the laws of the animal organization. It is from an utter forgetfulness, or contempt, of this essential principle, that the science has been infested by so many chemical and mechanical theories. The humoral pathology rested on a fallacious and shallow conception of the laws of life. The nervous system was [and is] overlooked, its structure disregarded, and its laws unobserved."

"What has humoralism ever done for medicine? It has retarded its march, it has shut up the avenues to its successful cultivation, and deteriorated the genuine spirit of correct investigation."

"The doctrine of the corruption of the blood in diseased states of the economy is amenable to one unanswerable objection; — this corruption has never been demonstrated. It is surely not logical to contend for the possibility of a thing which has never been proven, [or shown to be possible.] We are not, as medical philosophers, to establish any of our views of morbid action on mere possible contingences." — "This concession of Cullen bears Sir Gilbert Blane out in the declaration that — '*the whole of the humoral pathology rested on a fallacious, and shallow, though specious foundation.*'"

"Andral candidly avers — 'that, in the present state of the science, it is the part of a sensible man not to adopt the doctrine of humoralism too lightly, by judging from facts, many of which require re-examination before they are finally admitted, and that we ought to be particularly on our guard against being in too great a hurry to make practical applications of it.' (*Anat.* vol. i, p. 419. See *Comm.* vol. i, p. 627 — 641.)

"A doctrine so mysterious and impracticable, which must be thus guarded, limited, and dreaded, lest we rashly employ it for any useful practical purpose, cannot, surely, derive its existence from correct and enlightened experience; and, therefore, if we desire to see the reign of a just philosophy extending its healthful protection over the science of medicine, we should renounce a scheme of explanation of disease, which is not founded on sound observation, and which exerts such dubious, if not dangerous, tendencies over the practice of our art."

"Dr. Armstrong's popular doctrine of congestion is founded on wrong views of anatomy and physiology, and is therefore untenable. Indeed, at page 288 of his work on Typhus, under the head of common continued fever, we have an admission that congestion and inflammation are the same."

"The Broussaian pathology of fever is amenable to a just objection on account of its generality." "Such pathological views involve no laborious process of thought, and demand no extensive examination. They are light and portable, easily transferred from the author's page to the reader's comprehension. The memory alone is exercised in their reception." (*Prof. Harrison's Lectures*, p. 39 — 49: 1835.)

It is a doctrine, however, of constant recurrence in the Essay on the Humoral Pathology, that the blood is *always absolutely* more or less *diseased* when the solids are affected, whether generally, or *locally* if to much extent. Thus:

"The solids, which give being and vitality to the blood, become, in their normal state, it is said, the subject of its morbific action; and, according to the PREMISES of humoralists and SOLIDISTS, *when the solids are diseased* the BLOOD UNDERGOES DISEASE *in consequence*; and, since, [according to the humoralists] the blood was *originally* the cause of the morbid action of the solids, *every increasing degree of disease, according to the admitted premises, must be a cause of increasing disease in the solids.* This must be equally true of LOCAL as of constitutional diseases. No portion of the blood can be, long, morbidly affected more than the whole mass; and since, when universally diseased, it should produce one universal disease of the solids, it is manifest, from the constant occurrence of *local* affections, that humoralism is striving against the plainest evidence." (*Comm.* vol. i, p. 647. *Reply*, p. 52.)

Again the Commentaries:—"We shall notice here an affirmation by M. Andral, since it is regarded by high authorities as nearly closing the door against farther discussion. 'Physiology,' he says, 'leads us to the conclusion that every alteration of the solids must be succeeded by an alteration of the blood, just as every modification of the blood must be succeeded by a modification of the solids. Viewed in this light, there is no longer any meaning in the disputes between the solidists and the humoralists.'"

"This is any thing but a fair statement of the great question at issue. It is not whether the blood becomes DISEASED by a morbid action of the solids; and the solidist is surprised that the defence of humoralism should often turn upon laboured attempts to prove WHAT EVERY BODY ADMITS. (*Reply*, p. 52—53.) Nor is it, whether vitiated blood, or putrid matter, will excite disease when injected into the veins. The question at issue is *whether foreign morbific causes, and remedial agents, in their ordinary modes of operation, produce their primary effect upon the solids or upon the blood, and the latter become the cause of disease in the former; whether we 'have hereditary humours, as gout, scrofula, etc.,'* and *whether we are 'the parents of our own humours, and that we breed bad humours;'* whether they 'gravitate to the legs,' or are 'brought to a part by poultices;' whether, according to Andral, 'those derangements of functions and organs *produced by the experimenter*, when he introduces different deleterious substances into the blood, are *likewise those* that are produced by the sting or the bite of certain animals, and are also *those* that take place in smallpox, measles, and scarlatina, and are the *same* derangements that appear in persons exposed to putrid emanations, vegetable or animal, and to miasmata from the bodies of other persons that are themselves diseased and crowded in confined places, the *same* which show themselves in individuals whose blood is only imperfectly or badly repaired by *insufficient* or *unwholesome diet*;' whether, in other diseases, 'where *no deleterious substance* has been introduced into the blood, and in which there is no direct proof that any alteration of that fluid has been the primary cause of the morbid phenomena,' but where the symptoms and morbid appearances may have some resemblance to those of the foregoing affections, 'it appears, as in the preceding cases, the *primary* cause of disease should be referred to the blood,' and whether 'the whole blood must be altered or corrected' by 'incisives, diluents, attenuants, inviscants, incrassants, revulsives, repellents, concoctants, deflectants, derivatives, depuratives, deobstruants, detergents, agglutinants, incarnatives, refrigerants,' etc.? These are the questions."

"But the most objectionable part of M. Andral's statement, by which he calmly identifies solidism and humoralism, is the assumption that the blood is admitted by the solidists to be 'modified' without the agency of the solids, and to become, in consequence, the cause of disease in the latter; or, in the words of our author, 'just as every modification of the blood must be succeeded by a modification of the solids.' We shall not dwell upon this *coup de main*," etc. (*Comm.* vol. i, p. 636—637.)

The author often dwells upon the necessity of a more or less "*diseased*" state of the blood in every diseased condition of the solids, and perfectly concurs with M. Andral, and twice adopts his language, that,

"No one solid can undergo the SLIGHTEST modification, without producing *some derangement* in the NATURE or quantity of the materials destined to form the blood, or to be separated from it." (*Comm.* vol. i, p. 630.)

As to the last clause in the quotation from the review, the author has much to say as to the state of the blood in "*local* diseases," though it is manifestly the purpose of the reviewer to give the impression that his author had overlooked the considerations relative to that question. Take, as an example, the following passage: —

"If we suppose, that in any serious LOCAL inflammation the blood becomes more or less altered, according to the foregoing principles, is it asked why the universal mass, being thus modified, is not detrimental to other parts? It is an obvious answer, that all other parts are now modified in their powers and functions by the sympathetic influences of the local affection. In proportion as that affection is capable of modifying the blood, so does it exert its sympathetic influence over all parts of the organization. 'Confluxio una, conspiratio una, consententia omnia. Juxta totius quidem corporis naturam omnia; juxta partem vero partes in unaquaque parte ad opus.' (1) The modifications of the blood, and the constitutional derangement, being produced by a common cause, the blood and the solids are universally adapted to each other. It matters not, therefore, how 'black,' 'woolly,' or dissolved, the blood may become in 'scurvy,' and 'putrid' fevers; and that such patients ever recover is especially owing to the absence of healthy, stimulating blood."

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(1) Hippocrates, de Aliment. ver. 45.

"Nature has endowed the living organization with numerous resources for its protection; some of which may be habitually dormant, but are called into action by many accidental causes that would constantly endanger life without them." (*Comm.* vol. i, p. 655.)

The Essay on the Humoral Pathology being falsified in less than a page, we come next to that upon Animal Heat, to which about four pages are devoted. The principal theoretical disagreement between the reviewer and the author consists in the former attributing the development of heat from the blood to "chemical changes," whilst the latter ascribes it to a *vital* process.

It is astonishing that a contributor to the British and Foreign Medical Review should not be aware that Hunter, Bichat, and other eminent vitalists, consider the elaboration of heat from the blood a process analogous to "secretion." But here again he gives to his author the credit of the discovery. Bichat is so emphatic upon the subject that he says, (what is worth quoting for other purposes,) that,

"It seems to me, that the explanation which exhibits nature always pursuing an uniform course in her operations, drawing the same results from the same principles, has a greater degree of probability than that which shows her separating, as it were, this phenomenon from all the others, in the way which she produces it." "The extrication of caloric is a phenomenon *exactly* analogous to those of which the general capillary system is the seat." "When we place upon one side all the phenomena of animal heat, and on the other the *chemical hypothesis*, it appears to me so inadequate to their explanation, that I think every methodical mind can refute it without my assistance." (*Bichat, ut cit.* vol. ii, p. 46; and *Reply*, p. 18.)

It is well known, also, to most people, that Hunter maintained that — "the power of generating heat in animals is that power which preserves and regulates the internal machine." (*Hunter's Observations on the Animal Economy*, p. 91. 1786.)

The reviewer supposes that there can be

> "No essential difference between Dr. Paine's hypothesis and that of Dr. Crawford; since this pre-existing heat [put into the blood by the reviewer] exactly corresponds with the *latent caloric* of the chemist, which only requires certain *chemical* changes to render it sensible." (*Rev.* p. 394.)

The author will make no comments upon this perverted representation of his doctrine; but will ask the reviewer to define the nature of heat, and his views as to its origin? The *laws* of inorganic and animal heat are pretty well ascertained; and will the reviewer explain the *cause* of their difference? So great a philosopher as *Moore* remarks, that,

"We must allow the bodies of living animals and vegetables to form an *original cause* of heat, as much beyond our power of explaining as the source of the sun's heat." (*Moore's Medical Sketches.*)

There is one remark, contrasting the doctrine of the reviewer and the author, of which the former shall have the benefit in his own language, — a fairness in which the author takes much pleasure, though a very rare opportunity conceded to the author. Thus:

> "In Dr. Paine's opinion, then, as in ours, the *acknowledged influence of the nervous system upon the production* of animal heat is exercised through the medium of the organic functions; and the difference between us consists in this, that *he* regards it as one of those functions incapable of being explained by any other than *vital laws*, — which, in the present state of our knowledge of *those laws*, is *equivalent* to saying that we know NOTHING AT ALL ABOUT IT; whilst *we* consider it as a *result* of those functions produced by the MOLECULAR CHANGES which they involve, — *these changes* being themselves governed by the ORDINARY LAWS OF CHEMISTRY.'' (*Rev.* p. 396.)

From the coincidences hitherto indicated between the doctrines and language of the reviewer and Dr. Carpenter, the inquisitive reader may be interested with knowing that the same parallel exists between the reviewer's remarks on animal heat and those of Dr. Carpenter's "*Principles*," &c. p. 377—378.

The reviewer is peculiarly intent upon the doctrine of respiration, and complains that his author did not add to the *bulk of his work* by noticing, more fully, Newport's observations as to the respiration of insects. But, the main object of the Essay is to disprove that doctrine, and the author only felt it necessary in respect to the experiments upon insects, to refer ACCURATELY to the fact that Newport had *connected* their *generation of heat* with the process of *respiration;* which he did in the following manner: —

"He (Newport) also found that the power of generating heat is exalted during their breeding season; though it might have been more difficult to ascertain that the amount of heat evolved IS IN PROPORTION TO THE QUANTITY OF AIR RESPIRED." (*Comm.* vol. ii, p. 66.)

Now observe the *characteristic* statement of the reviewer: —

> "A stimulus, which excites the individual [an insect] to activity, also increases the number of its respirations and its consumption of oxygen; and the temperature is raised, as Mr. Newport has shown, [?] EXACTLY IN THE SAME PROPORTION. Now, in *alluding* to Newport's experiments, Dr. Paine ENTIRELY OVERLOOKS THIS FACT"! (*Rev.* p. 395.)

Our conscientious and verbal critic, —

> "Will not impute the mistake of calling the separation of the oxygen of the atmosphere from the nitrogen *mixed* with it an act of *decomposition* to Dr. Paine's *ignorance* of the meaning of chemical terms." (*Rev.* p. 393.)

Nor, on the other hand, will Dr. P. impute to the reviewer an "ignorance" of the fact of its being still *sub judice* whether the oxygen and nitrogen are simply *mixed* or *chemically combined.* Nearly a page is devoted to showing that the author's experiments upon the temperature of trees are not as minutely "recorded" as John Hunter's. The material objection is that the temperature of the earth was not ascertained sufficiently low down. The experiments, however, as it regards the purpose of the author, are in no respect invalidated. The temperature of the earth was ascertained as low as the roots of the smaller trees extended, and as theirs was almost always the highest temperature, and as those whose roots reached lowest down had the lowest temperature, the necessity of going deeper with the thermometer, so far as respected the principles the author attempted to illustrate, was superseded. All the experiments, too, at each observation, were regulated by common standards, and the detail, therefore, required by the reviewer would have unnecessarily increased his objection to a "great book."

Page 397 is devoted to a *denial* of any organization or vitalization of the food until it is "taken into the vessels."

> "The ORGANIZATION and *consequent* VITALIZATION of this substance COMMENCES AS SOON AS, being taken into the VESSELS, it is admitted into the living system." (*Rev.* p. 397.)

And so Dr. Carpenter : —

> "With the process of ABSORPTION, strictly so called, the ORGANIZATION of the constituents of the alimentary fluid, and their ENDOWMENT with VITAL PROPERTIES may be regarded as COMMENCING in animals as well as in plants." (CARP. *Princip.* p. 206.)

And yet these *identical* writers "ASSERT," that the very "ELEMENTS of matter are ENDOWED with *vital properties.*" (See *Reply*, p. 37—38.) Which way shall we have it?

Even so great a defender of the *chemical digestion* of food as Dr. Prout fully admits an organizing and vitalizing agency of the stomach; (1) for which he gets a reprimand in another article in the present number of the British Review, (p. 445.) whilst in yet another article, it is said that, in a later work, (2)—"Dr. Prout maintains that the stomach possesses a *vitalizing* and *organizing* faculty, whereby it is enabled to fit the crude aliment for contact with the living structure." (*Rev.* of Prout, p. 332.) Where does that "*vitalizing* and *organizing* faculty" reside? Is it certain that the stomach, or the vessels, can *vitalize* any better than the gastric juice, which is especially secreted for the purpose? But, Dr. Prout, along with Liebig, and even Müller, (1) are marked for the sacrifice by philosophers who complain most loudly of "transcendental vitalism;" though their chemical and physical theories of life are duly recognised as "conformable to the present advanced state of knowledge." (2) But, take the following passage from *Liebig* as one of those scintillations which illuminate and betray the errors of a master-mind.

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(1) Prout's Chemistry, Meteorology, and the Function of Digestion, considered with Reference to Natural Theology, b. 3, c. 3. (2) On the Stomach and Urinary Diseases, 1840.

"The individual organs," he says, "such as the STOMACH, cause all the organic substances conveyed to them which are capable of transformation to assume new forms. The STOMACH COMPELS THE ELEMENTS of these substances to unite into a compound *fitted for the formation* of the blood. [Can artificial '*pepsin*' do as much?] But the *blood* possesses *no power* of *causing transformations.* On the contrary, its principal character consists in its readily suffering transformations; and no other matter can be compared in this respect with it." (*Liebig's* Organic Chemistry applied to Physiology, &c. p. 346.)

The chemical hypothesis of digestion is *affirmed* by the reviewer; but there is no attempt to show its validity, or to indicate an error in the author, except to class him with the "transcendental vitalists." The artificial mixtures of "Müller, Schwann, Eberle," are duly approved; as they are also by Dr. Carpenter. Thus the latter:—

> "SIMILAR effects [to those of the gastric juice] have been obtained by an *artificial* GASTRIC *juice* [!] which has been formed, by Muller and Schwann, of a mixture of dilute *acetic* or *muriatic acid*, with *mucus* of the stomach; the simplest way of MANUFACTURING it being," &c. (CARP. *Princip*. p. 208.)

The author's doctrine is presented after the usual manner of the reviewer, where "vitalism" is concerned. He represents the author as "mixing up the vital properties with the gastric juice," in the way in which we have just seen them "mixed up" by those erudite *chemists*, Prout and Liebig, and forgetting that he and Dr. Carpenter had just "mixed up" the same properties with the chyle and blood, whilst Dr. Carpenter also endows the *fluid* ovum with *such* vital properties as are capable of unfolding, and without chemical agencies, all the essential parts of the fœtus. (See *Reply*, pp. 32—33, 40.)

"We have next," says the Journal, "a learned review of the Theories of Inflammation," (p. 398.) The only apparent objection to this Essay is the author's doctrine that this "disorder consists in disorder of the vital forces;" (*Reviewer's phraseology*,)—a rather lame objection for one who defends the humoral pathology by affirming that "the *vital* properties of the blood are capable of *alteration* by various causes." And this leads the author to say that he is greatly misapprehended by the reviewer in supposing that the change of those properties in inflammation consists alone in their "*exaltation.*" The author supposes them, also, to be otherwise "altered," just as his reviewer so correctly supposes of the "vital properties of the blood," when that fluid is diseased. There is no other attempt to invalidate this Essay.

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(1) See CONDEMNATION of Muller's "*vital principle* or *organic force*," in British and Foreign Med. Rev., Jan. 1839, p. 172. The second English edition, however, of Muller's Physiology is said to be expurgated. See *Ibid.* January, 1840, p. 244.

(2) "The British Association for the Advancement of Science," 1838, deputed a committee of those eminent chemists, Thomson, Prout, and Graham, with whom Prof. Owen was also associated, to make experiments upon the gastric juice of Alexis St. Martin. Now, we respectfully submit whether the foregoing inquiry does not fall under the province of the physiologist? (See *Reply*, p. 54. Extract from Review.)

As the review naturally divides itself into two parts, so also should the examination. That portion which has been the subject of analysis is without a clause to mitigate its rank injustice. But, continued misrepresentation would have been exuberant when it ceased to be useful; and few are so obtuse as to carry it beyond the limit of satiety. The reviewer, therefore, having thus disposed of vitalism and solidism, appears to have abruptly concluded, on reaching the other Essays, that it might be as well to have some reference to a sense of ordinary justice and decorum. What remains to be said, therefore, is defensive only in a passive sense. Folly and Ignorance are not the usual companions of "much power of mind," "great learning," "zeal, and industry," which the author concedes are verbally admitted far beyond his desert by the reviewer; nor could they achieve what the subsequent Essays are allowed to embrace, without leaving a trace of their counter-influence upon that equal bulk of the work, in which the reviewer sees nothing to impugn but by misrepresentation.

"The next Essay, the Philosophy of Venous Congestion, is, we think," says the reviewer, "altogether the most valuable in the work." He also thinks "that the author is quite right in *asserting* [showing] that, in many instances, the enlargment of the veins and the STAGNATION [?] of the blood in them cannot be accounted for on the ordinary theories, and that they are due to some change in the veins themselves. We are farther disposed to admit the probability that this change is of an inflammatory character." "Having established in his own opinion that 'venous congestion [true congestion] is in no respect mechanically determined by a remora of blood,' he proceeds to show, and we think with more success, that a mere passive relaxation of their parietes will not produce it, and that the cause is to be sought in some morbid condition of the trunks themselves. This he regards as of an inflammatory nature; producing varicose enlargements when local and chronic, and giving rise to congestive fevers, [not to the *fever*.] A considerable body of evidence, from symptoms, post-mortem appearances, and the effects of treatment, is brought forward in support of this view; and we are disposed to recommend it strongly to the attention of our readers." (*Review*, p. 398—399.)

Nevertheless, the acerbity of feeling is not yet assuaged. The author has made no greater compromise with the mechanical hypotheses of venous congestion, than he had in his former Essays with the chemical and physical doctrines of organic processes. "*Excessive dogmatism and exclusiveness*," and other unseemly epithets, continue, therefore, to be favourite modes of representing the author; nor is there wanting that more *personal* offence, of which examples have already been presented in attempts to degrade the author by distorting his views in the language of insult.

A page (p. 399) is occupied with *entirely perverting* what the author has said in respect to the independence of true cerebral congestion, or even mechanical plethora of the cerebral veins, of *remora* of blood in pulmonary or other thoracic affections, unless, according to the author in obstructions of the jugular veins or vena cava. Compare that page with *Comm.* Vol. II, pp. 249, 252, 262, 482—483, 217 *note* (8) 427—428, 232, 248. And, notwithstanding the author has quoted the works of FIFTEEN writers to *prove* a *mechanical* injection of the veins of the brain, in cases of *man* "HANGING," (*Comm.* Vol. II, p. 255), and has much to the same effect, (pp. 252—254, 238—246), the reviewer charges him with ignorance upon this subject, after the following manner:—

> "Surely *hanging*, whether judicial or suicidal, is not so rare an occurrence in America, that Dr. Paine has never witnessed its post-mortem phenomena." (*Review*, p. 399.) — [The reviewer *directly* and *utterly perverts* his author's whole demonstration and facts, as to the absence of cerebral derangement in NATURAL "obliterations of the cerebral veins and vena cava," by this *management*.]

The reviewer could also "give the Essay higher commendation, if it were compressed from *four hundred* pages to *forty*." (*Review*, p. 398.) But the author had embarked upon an unexplored region, and one, if he were right, of vast moment to mankind; he had prejudices and malevolence, like his reviewer's, to contend with as in all the other Essays: numerous difficulties were to be removed; artificial experiments to be exposed and divested of influence; the facts and opinions of great philosophers to be respectfully examined, etc. Various other specific questions, such as the pathology of varix, of venous hypertrophy, of purpura hemorrhagica, of the true asthma, of the cerebral congestions which spring from alcohol, the narcotic poisons, cold, etc.; the diversities and complications of active phlebitis; the radical distinction between inflammation and idiopathic fever, and the modifications of each by venous congestions, and how each disease is modified by various predisposing causes; the pathology of the true puerperal fever; the powers which govern the circulation (*venous* and arterial); and a variety of other topics indispensable to the great object of the Essay, are so considered as to form distinct disquisitions in themselves, whilst they are relatively intended for a consistent whole. May not the facts, also, which the author has accumulated here and in other places, be of some advantage independently of the principles they are designed to illustrate?

Events have only satisfied the author that he should not modify his plan; and, when another edition shall be published, instead of abstracting from the present, other materials will be added; and, that a proper issue may obtain between the reviewer Dr. Carpenter, and the author, the present examination will be incorporated with the work.

"The next Essay," says the reviewer, "on the Comparative Merits of the Hippocratic and Anatomical Schools, contains many sound remarks on the absurdity of the pretensions of the Modern French School of morbid anatomy, and the necessity of the observation of the phenomena of disease in the living state for the success of medical practice." (*Review*, p. 400.)

"The work concludes with a severe review of the writings of Louis, in which Dr. Paine points out certain alleged fallacies of his method of generalizing, and exposes his hasty condemnation of previous observers. It may be thought a little strange that *Dr. Paine should see these faults so glaringly in another, but should be so utterly unconscious of them in himself; but alas!! for human nature, such a self-delusion is by no means uncommon.*"

["Oh! that *thou* wouldst the giftie gie us
To see oursels as ithers see us."]

"He seems to regard M. Louis' system of observation as not only useless, but strongly injurious; and he does not give him, by any means, sufficient credit for the mass of valuable materials which he has collected for the benefit of those who *can* use them aright." (*Review*, p. 401.)

The author has marked the characteristic sentence in the foregoing quotation for the purpose of comment. It embraces insinuations which are only worthy a mind that was capable of the injustice which has been hitherto exposed. Throughout the "Commentaries" the author has dwelt upon the importance of general principles in medicine, and the *whole object* of his work is to attempt a reduction of scattered facts to such principles. Nor has he been guilty of the alleged inconsistency of objecting to this practice in others; but, on the contrary, in numerous places has he urged its importance, and lamented its neglect. (See, particularly, *Appendix* on *Analogy* and *Principles in Medicine*, Vol. II, p. 574 — 589.) As it respects M. Louis, the object of the author was *totally different* from what is implied by the reviewer,—it being to exhibit a "glaring" inconsistency in M. Louis of condemning all "generalizing" by others, while he himself was addicted to the practice beyond all medical philosophers. As to the implied charge that the author has "hastily condemned previous observers," he can only make a positive denial, and, by associating this reply with the work itself, commit the issue to an impartial public. The author will here have one word as to his respect for the writings of Hippocrates, Aretæus, Celsus, and other fathers of medicine. His main object was to institute a contrast between their habits of observing nature and the artificial systems of more recent times. Like all other sane men, he believes the human mind to be capable of progression in knowledge, but also believes that this progress may be impeded or arrested by false philosophy, if he may so call it. The author's sentiments upon this question are fully expressed in Vol. II, pp. 676—677, 806—815.

Finally, from what has been now seen, what must we conclude from the following, and other passages of a similar import, which occur in the review?—

> "As on this subject we formerly expressed our *complete accordance* with Dr. Carpenter's positions, whilst differing from him on other points, we shall in our present observations *identify* our own with them, more especially as Dr. Paine's criticisms are directed to both alike." (*Rev.* p. 387.)

Now, lest some subterfuge be concealed in the foregoing asseveration, will the reviewer of Dr. Paine's "Commentaries" distinctly avow that he also wrote the review of Dr. Carpenter's work? (1) Will the editor assume the responsibility? (2) But, however this may be, there can be no doubt that the author of the "Medical and Physiological Commentaries" has met a proud adversary in ambush, and stamped him with indelible marks of *falsehood* and *prevarication.*

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(1) The following is the title of the work to which the author refers:—

"PRINCIPLES OF GENERAL AND COMPARATIVE PHYSIOLOGY, BY WILLIAM B. CARPENTER, MEMBER OF THE ROYAL COLLEGE OF SURGEONS, LONDON; LATE PRESIDENT OF THE ROYAL MEDICAL AND ROYAL PHYSICAL SOCIETIES; AND FELLOW OF THE ROYAL BOTANICAL SOCIETY, EDINBURGH; AND LECTURER ON FORENSIC MEDICINE IN THE BRISTOL MEDICAL SCHOOL.—*London*, 1839."

(2) If Dr. FORBES will compare the review of the *Character and Writings* of JOHN HUNTER, which appears in the April No. 1839, of the *British and Foreign Medical Review*, with the REV. DR. CHANNING's eloquent "*Remarks on the Character and Writings of* JOHN MILTON," and then look at an Article in the April No. 1839, of the *Edinburgh Review*, upon Dr. Channing's "*Remarks*," he will see the advantage of requiring initial signatures from the contributors to his Journal; whilst such articles as appear anonymously would be duly accredited to the Editor, and his correspondents thus protected. He need not go beyond pages 418, 419, 420, (*a*) 422, 423, of the Medical Review to appreciate the force of the foregoing suggestion. The author could assign not a few other *similar* reasons,—but this for the present.

Before the reader, however, shall have done with this inquiry, let him *critically compare* the following pages of the article in the *Medical* Review, with the following sections of DR. CARPENTER's "*Principles*," *etc.*; namely, pages 426—427, 433, "The human body," etc. with sections 512, 513, 514, 515, 5; and pages 430, 332, with section 364. See, also, Dr. Carpenter's "*Preface*," p, 7—8.

The author has quoted his reviewer, (*Reply*, p. 41), as saying, that in another article HE had "formerly shown the blood to be endowed with vitality." That article is in the July, (1839), number, and is a *continuation* of the *elaborated* review of the *Character and Writings of John Hunter*. (See *Review*, p. 177—183.) Here, also, compare *Channing* with page 183, &c. "What a world of thought," etc., "we are naturally curious to know," etc.

"His reviewer read for METAPHYSICS under the letter M, and for CHINA under the letter C; and combined his information, Sir."—(*Pickwick Papers*.)

"We have been induced to make these GENERAL REMARKS to save us the necessity of EXTRACTING LARGELY from a work," etc. (*Rev.* April, 1839, p. 422.)

(*a*) "The whole community is now turned into (MEDICAL) readers"! (*Channing* and *Reviewer*.) See other similar metamorphoses.

> "The time hath been when no harsh sound would fall
> From lips that now may seem imbued with gall."—*Byron.*

The author in submitting this "examination" to his professional brethren, regrets that his engagements have rendered it necessary to execute it with greater haste than he could have desired. He has now only to add, that he has endeavoured not to neglect any criticisms of the reviewer, and to omit nothing which was intended to operate disadvantageously to the "Commentaries." An apology is scarcely necessary for the Italics and Capitals which abound in the "examination," since, the reviewer's *criticisms* being of a *verbal* nature, it became necessary to indicate the perverted words, and their true import, by appropriate marks.

In having thus, for the second time, exposed the malevolence of those who have felt annoyed by the author's criticisms, and, as there are many others, particularly in Europe, from whom the author has had the misfortune to differ in opinion upon important doctrines, he feels entitled to an impartial consideration of any strictures upon his "Commentaries" which may appear hereafter; and, in the language of Dr. Carpenter's successful appeal, "he will only now express the hope that, as *his* inferences have not been formed hastily or inconsiderately, they may not be too readily pronounced crude or unphilosophical." (CARP. *Princip. Preface.*)

NEW-YORK, May 5th, 1841.

The introduction of the following extracts is in no respect intended either to sustain the foregoing defence, or to advance the interests of the "Commentaries." So far as the reviewer, Dr. Carpenter, and the author are relatively concerned, the latter desires that their respective merits should rest entirely upon their own ground. But, the author cannot forbear connecting with this pamphlet a memorial of the kindness which he has received from the medical press of his own country. It may be also as well to premise that the author is personally unacquainted with every editor of the American medical periodicals, excepting the gentlemen who conduct the "New-York Journal of Medicine and Surgery." To those who have done the author the honour of the following notices, (and he believes that the writers are all, with the foregoing exceptions, personally unknown to him,) the author embraces this opportunity of conveying his profound gratitude. Perhaps, indeed, in making the foregoing "examination," he has been as much influenced by a sense of obligation to his unknown friends as by any other consideration.

"No further evidence than these volumes is necessary to show that Dr. Paine is a man of extensive and varied erudition, whose industry has never been surpassed in the annals of American medical science. It would seem at first view, that the man who had written these huge volumes, aside from the thousands of references to all authors of credence from a remote antiquity, minutely and accurately registered in the margin, could have done hardly anything else in the course of a long life. Notwithstanding the praise and respect due his high literary attainments, and his profundity in the circle of the sciences, we imagine Dr. Paine will be dealt with severely by the medical press of this and other countries. There are a score of sins and medical heresies to be detected by the right worshipful admirers of Louis, which will annoy him hereafter." — *Boston Medical and Surgical Journal, July*, 1840, p. 383.

"This work reached us so late as to afford time merely for a glance at its contents; but this superficial examination has most favourably impressed us in relation to the learning and industry of the author." "But it is not only for the learning it displays, that the production of Dr. Paine may be recommended. The language and style, as well as the skill and ingenuity with which he maintains his own opinions, and contests those of others, show him to be a scholar; &c." — *The American Journal of the Medical Sciences*, *Phila.*, *Vol.* 26, 1840, p. 437.

"We have received this book of Dr. Paine with great pleasure. It has been long expected. The mechanical execution is beautiful, and the contents, as was to be expected from the well-known character of the author, are replete with value." — *The Maryland Medical and Surgical Journal, Baltimore*, *July*, 1840, p. 365.

"It may be said, that many of these subjects are not of a *practical* nature; and to those who regard the mere pouring of drugs, of which they know little, into a body of which they perhaps know less, this may be the fact. We trust, however, that at the present day there are few such. Some there are who doubt the utility of pathology, laugh at auscultation and percussion, and consider that no improvements have occurred in medicine of late years — because they have not instituted or are ignorant of them. Such persons, likewise, may regard the dissertations before us as of but little value; but to the large mass of physicians of the day, who are anxious to improve their profession in the only way in which it can be legitimately and signally improved — that is, by a proper attention to physiological, pathological, and therapeutical principles, we can recommend them as essays replete with information, the perusal of which cannot fail to expand the mind, and to lead to trains of thought pregnant with benefit to the professional reader himself, and through him to the community.

"On many of the subjects, our views are by no means in accordance with those of Dr. Paine, but his essays have not been the less welcome on that account. He is liberal, well read, argumentative, and candid; and his volumes exhibit, that his attention has not been directed merely to medical lore, but that his literary qualifications are ample also."

"Surely the intelligent author does not mean to convey the idea, that he is the first to treat of 'the philosophy of the operation of blood-letting.' We admit, that, *ex professo*, essays on the subject are rare; yet we had fancied, that we ourselves had added an humble contribution on this subject some years ago, and in a volume, which the author has done us the honour to cite, but which, — as it respects this topic, — has not attracted his attention." [Prof. Dunglison's General Therapeutics, p. 396 to 428.]

"Certain of the author's opinions we shall doubtless have occasion to advert to hereafter. In the meantime, we advise all to peruse the work for themselves, which contains a vast fund of information agreeably conveyed." — *American Medical Intelligencer, Phila., July*, 1840, p. 117.

"They are disquisitions on important questions in physiology, pathology, and therapeutics, with an amplification of argument, and variety and precision of bibliographical details, which, in this age of microscopical examination on the one side, and transcendental vagaries on the other, is truly instructive and refreshing. It is easier, by far, to praise and admire the laborious research of the author than to imitate him; but let not the inability to do the latter prevent the former, which Dr. Paine may rightfully claim as his due. If the 'Commentaries' afford, as they do, food for the 'mind contemplative' of the learned physician, they will also, and herein we would especially recommend them, habituate by their perusal the young student, and the yet imperfectly read practitioner, to inquiry and reasoning on the phenomena of life and the modifications to which they are continually subjected. Our recommendation of this work would be nearly as strong even if we differed from the author in some of his conclusions, as it would be if these were entirely in coincidence with our own opinions. We deem the perusal of his essays a healthy and strengthening exercise, in the taking of which, much good will be gained in the course of the journey over his pages, many new facts acquired, and pregnant hints offered, even though at the end, the traveller may not acquire possession of an El Dorado, or a mine with unalloyed gold."

"We must rest content, for the present, with pressing them on the attention of that class of medical men who may be the least inclined to encounter a continuous perusal of the two large volumes of which the work consists. But they are not obliged or expected to read them through at once. Let them rather take up one of the important themes discussed and study it with care; and then after a due interval, we will not prescribe its duration, go on with another. Every hour honestly devoted to a study of the work will create, we are sure, fresh desire, and, what is more, will give increased ability for farther progress over its richly adorned pages." — *Eclectic Journal of Medicine, Phila., August*, 1840, p. 382.

"The features of the work before us are peculiar. Directing his attention almost exclusively to questions of physiology and medical philosophy, Dr. Paine has, throughout his several memoirs, endeavoured to show the direct application of his principles to the practice of the healing art. He has touched on almost every disputed or unsettled point in medical philosophy."

"Every inquiry into the laws that regulate the actions of living bodies, he maintains must turn wholly upon the vital forces. And, says our author, 'he who shall regard them as coincident with the powers that rule in the inorganic world, must, as appears to us, travel in a route upon which he will be forever losing his way.'

"How just is this remark when applied to what has of late years assumed so much importance, under the specious show of experimental philosophy!"

"They are, nevertheless, daily gaining ground; and the bold attempt to substitute mere physical forces for the vital power, in the study of organic actions, which has recently been made by the celebrated Magendie, in his lectures on the blood, is a sufficient evidence, that at the present moment, vital philosophy is in need of an advocate, able and willing to defend it. Such an advocate is our author." —

"In conclusion, we would recommend the study of these volumes to the profession. Few medical men, even amongst our most learned and experienced, can peruse these essays without benefit, or without receiving information that may be of avail to them in future. We know of but few works, and certainly none in the range of American medical literature, to compare with this, for the extent and variety of professional research evinced in it." — *New-York Journal of Medicine and Surgery, July*, 1840, p. 146 — 172.

"The review passed unavoidably into new hands." "The more we examine the work, the more are we astonished at the immense research it displays, the more we are pleased with the general fairness and candour of its statements. To this latter expression, however, we are compelled to state that there are some striking exceptions; a mistake into which Dr. Paine has unintentionally, no doubt, been led by his enthusiastic attachment to what he believes the true principles of medical philosophy. None among his warmest admirers are more convinced than ourselves of his ardent love of truth, or of his devotion to the improvement of medical science; the work before us is a stupendous proof of much more even than this, to those who read it with the attention it deserves."

"Equally obscure to our minds is the true pathology of venous congestion; yet the path is clear, and for this we are indebted to Dr. Paine. His investigations have thrown great doubt on pre-existing theories: they have called forth a new view of the whole question; the truth of which is not to be tested by empty speculations, but by careful examination and patient thought."

"That our author is perfectly sincere and honest in his condemnation of M. Louis, we have no doubt whatever; but we regret none the less the attempt he has made to crush one whose position is so firmly fixed in our medical literature. The numerous admirers of M. Louis cannot indeed fail to be shocked as well as surprised at the bold denunciations that are poured out against him. They have this consolation, however, that the thunder which rolls so loudly does not shatter, and that while a few may pause and wonder, none will be convinced. In making these remarks, we would not be guilty of undervaluing Dr. Paine's labours."

"Of this we feel well assured that those who differ from him the most, will find much to admire,—many rich sources of instruction by which they may profit and improve." —*Ibid. Oct.* 1840, p. 403—432.

"The work, whose title is announced in the above heading, is one of the most remarkable contributions that has been made to the medical literature of our country.

"As an American production, not only is it unusual in its size and the abundance of its matter, it is still more unusual on account of the extent and elaborateness of research, especially in books, and of the great amount and diversity of learning, both ancient and modern, by which it is characterized; and, as respects the *latter* quality, we might safely add, *enriched.* Indeed in both qualities, if we mistake not, it may be pronounced *unique* in the medical annals of the United States. And in standing sufficiently correspondent to these are the magnitude, and bold ambitiousness of its aim and object, namely: *To expound, vindicate, and determine some of the fundamental principles, and most important doctrines in the philosophy of medicine.*"

"Those members of the profession, who are desirous of attaining a thorough and competent knowledge of the work we are examining, must peruse it themselves; and not merely peruse it, but make it a subject of laborious and accurate study. In no other way can their desire be gratified."

"Under this head, (Vital Powers,) Dr. Paine, like a warrior, confident alike of the rectitude of his cause and of his own powers to maintain it, promptly shows his colours, and heralds forth in no doubtful terms the principles for which he is resolved to make battle. And battle he does very strenuously make from the beginning to the end, not only of the present essay, but of his two elaborate and massy volumes. Every 'section,' and almost every page of the entire work, is, more or less, an arena of conflict. To vary our style, rendering it less warlike, and therefore more appropriate to our subject and purpose, Dr. Paine is a controversial writer; and though we do not, as will appear hereafter, concur with him on every subordinate point he discusses, we consider him, as relates to most, if not all, his fundamental principles and general doctrines, correct and triumphant. The reason of his triumph is plain. He contends for TRUTH, as it is clearly, we think, expressed in the BOOK OF NATURE, is master of his own subject, and possesses ample means for its illustration and defence. He is, as will be presently perceived, a VITALIST and SOLIDIST. *And so, if we mistake not, is Nature herself.* His course, therefore, was straight and obvious."

"He has taken Nature for his guide, marked her footsteps, followed in her path, and faithfully employed, in vindication of sound principles in medicine, the all-sufficient means, with which she supplies her industrious votaries and enlightened advocates. And thus pursuing his purpose, with ability and perseverance, (and he is destitute of neither) his failure to accomplish it may be pronounced *impossible*. For the ultimate triumph of truth is as certain as any of the other Decrees of Heaven."

"We speak not here of *the positive* condition and character of the principles and laws that govern the world of living matter. These great sources and regulators of action and change are, *in themselves and in their relations to one another*, as certain, harmonious, and stable, as are any of the other elements or attributes of creation. And creation, being the work of an ALL PERFECT BEING, is free itself from imperfection and fault. The irregularity and unstableness apparent in it, are *only* apparent. They exist only in relation to ourselves, on account of the insufficiency of our knowledge, and comprehension of them."

"Without gravely attempting, at present, to arbitrate between two writers, one of them so able and learned as the American, and the other so popular and fashionable as the Frenchman, we say unhesitatingly, that we have long witnessed, with dissatisfaction and regret, the dense and unmeasured clouds of professional incense, that have risen to the latter, from numerous altars in the United States."

"What, we ask, has Mr. Louis done, for the real advancement of practical medicine, that entitles him to such homage? and the only reply, which truth countenances, is NOTHING! In point of example, he is proverbially one of the most unsuccessful practitioners in the metropolis of France, and that is a high standard of comparison. For, in that metropolis, practical medicine, if reports and statistics on the subject be not deceptive, is in a miserable condition, — a condition lower than in perhaps any other large European city — and greatly below its standard condition in the towns and villages of the United States."

"M. Louis, though professing to be an unprejudiced observer and interrogator of nature, and proclaimed as such by his pupils and followers, is one of the most confirmed theorists, and exclusive dogmatists, that has appeared in medicine. In his condemnation of the opinions and proceedings of his predecessors and cotemporaries, and in assuming that his own method of cultivating medicine *approaches* perfection, if it do not reach it, he is scarcely less sweeping and self-sufficient than Paracelsus In *his* estimation, all, or most at least, that is professionally valuable, centres in himself, and is incorporated in his '*numerical method*' and his *pathological discoveries*. According to his own estimate of matters, and that of his worshippers, he is the great medical Juggernaut of modern times, or of all times."

"We shall only add, under the present notice, an expression of our hope, that we shall find leisure hereafter to give such further analyses and expositions of Dr. Paine's 'Commentaries' as may be more gratifying and useful to the readers and patrons of this Journal, and more worthy of a work so erudite and able, as that we have been considering."—*The Western Journal of Medicine and Surgery; Louisville, Ky. April*, 1841, p. 257—270.

ERRATA. At page 15, first line, for *Graham* read *Daubney*. Page 17, *note* 2, in a part of the edition for 1824, read 1834. Page 18, forty-first line, for *morbility* read *mobility*. Page 40, line 46, before and after "not an organized tissue," erase marks of quotation.

THE MEDICO-CHIRURGICAL REVIEW,

April, 1841. *London.*

> "A man who by long consideration has familiarized a subject to his own mind, carefully surveyed the series of his thoughts, and planned all the parts of his composition into a regular dependence on each other, will often start at the sinistrous interpretations, or absurd remarks of haste and ignorance, and wonder by what infatuation his critics have been led away from the obvious sense, and upon what peculiar principles of judgment they decide against him."
>
> "Some seem always to read with the microscope of criticism, and employ their whole attention upon minute elegance, or faults scarcely visible to common observation."
>
> "Others are furnished by criticism with a telescope. They see with great clearness whatever is too remote to be discovered by the rest of mankind; but are totally blind to all that lies immediately before them."
>
> "When a book has been once dismissed into the world, and can be no more retouched, I know not whether firmness and spirit may not sometimes be of use to overpower arrogance and repel brutality. Softness, diffidence, and moderation, will often be mistaken for imbecility and dejection. They lure cowardice to the attack by the hopes of easy victory; and it will soon be found that he, whom every man thinks he can conquer, shall never be at peace." (JOHNSON's *Rambler.*)

Since the foregoing was printed, the April No. of the Medico-Chirurgical Review has fallen under the observation of the author. It contains a review of the "Commentaries," (p. 392—402,) in which there is something to excite the author's thankfulness; but there are many misapprehensions of a serious nature, which the author will rectify. In doing this, he will state all the criticisms.

The reviewer begins by saying that,—

"The contents are devoted to the consideration *of the most abstruse, metaphysical, pathological, and physiological subjects;* every page abounding with *references* to, or *quotations* from, authors of every age, and of every country;" &c. "It certainly is a favourable specimen of the laborious research and elaborate study of the writer. It reminds us of the German school of authorship; or of such books in our own language, as Burton's Anatomy of Melancholy; and puts to shame those medical authors who favour the world with the results of their experience, observation, and acquirements, without indicating always the SOURCES, *whence they have derived their knowledge.*

"For our own parts, however, we candidly confess that WE HAVE A PREDILECTION FOR THIS LATTER CLASS of writers": &c. (See *Reply*, p. 54—55.)

"The opinion Lord Byron had of the book, to which we have above alluded, as a literary work, *coincides exactly with our own, respecting this as a medical one:*— 'Burton's Anatomy of Melancholy,' said he, 'is the most useful book for a man who wishes to acquire the reputation of being well read with the least trouble. But, among the medley of quotations, the SUPERFICIAL reader must take care, or his intricacies will bewilder him. If, however, he has the patience to go through his volumes, he will be more improved for literary conversation than by the perusal of any *twenty books* with which I am acquainted; at least in the English language.'" (*Rev.* pp. 392, 393. See *Reply*, pp. 1, 65.)

Samuel Johnson, also, was wont to say, that—"Burton's Anatomy of Melancholy was the only book that ever called him from his bed an hour before his allotted time."

"In every section of his Essay, while he displays both industry of research and *dexterity* of *applying* quotations and references;" (1) &c. (*Rev.* p. 399.)

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(1) The Capitals and Italics are the author's, a liberty which the author has taken in common with his reviewer.

Were it not that the author's belief in the "immortality of the soul" has been impugned by the reviewer, he would have been quite disposed to allow the foregoing remarks to stand as a commentary upon the objections which occur in other places. The reviewer also does the author the honour of saying that, "*His work will be a lasting monument of the author's profound and multifarious reading*," — but there stands in repulsive contrast with this compliment the sentence immediately preceding, in which it is said that the work "*savours much more of the lamp than of the dissecting-room; and of the study rather than the bed-side of the sick*." (*Rev.* p. 393.)

We will soon see how far the foregoing conclusion is sustained by the proof which is offered by the reviewer; but, in the meantime, as to the *matter of fact*, the author will now put that at rest, by stating what is notorious amongst all his familiar acquaintances; that for more than a quarter of a century, his *days* have been *laboriously* devoted to the *practical* duties of his calling, and among those temperate classes of society where the best opportunities occur for pathological observation; whilst his literary labours (of which the "Commentaries" are not a moiety of what he has prepared for publication) have been mainly accomplished during those hours which are commonly allotted by mankind *to rest*. So far, the author concedes that "his work" may "savour of the lamp." As to the imputed neglect of "the dissecting-room," the author refers to his work on the "Cholera Asphyxia of New-York," to his Essay on Venous Congestion, and to the general character of the "Commentaries," that he is not obnoxious to the inconsistency of having neglected morbid anatomy. The author also in farther justice to himself will say, that, until the present season, though always toiling, it has been, for the last ten years, at the uninterrupted expense of health.

Let us, however, have the facts which have suggested the inference that the author has raised "a lasting monument of his profound and multifarious reading" by "the lamp," whilst he has squandered his days in idleness.

"We shall best enable our readers," says the reviewer, "to form their own opinion of the style and value of Dr. Paine's comments on other authors, and of *his own* PRACTICAL INFORMATION, by selecting a few of his criticisms on some of the more recent writers on bloodletting.—[*The author quoting without omission.*]

1. "Dr. Wardrop says, — 'The leading symptom by which the constitutional disturbance demanding venesection is indicated, will be found in the quality of the pulse.'

"Our author states, — 'There is scarcely any symptom, *per se*, that is less to be trusted to than the pulse, UNLESS IT POSSESS CERTAIN POSITIVE CHARACTERS.'" (*Rev.* p. 397. *Comm.* vol. i, p. 233.)

To which the author immediately adds, —

"One of these, *incompressibility*, Dr. Wardrop defines well. *Hardness* is another characteristic which *goes far* towards indicating the propriety of bloodletting, — since both are commonly the result of inflammatory action. But, the philosophical physician will be determined in all his remedies by the GENERAL ASSEMBLAGE of symptoms; especially local ones, if they exist. Again, the pulse is well known to be subject to the greatest variety of changes from *transient* causes." (*Comm.* vol. i, p. 233.)

The author has entered very largely into the consideration of the state of the pulse in venous congestions, and congestive fevers, and apoplexies, to show that it may, "*per se*," be a fallacious guide; and he flatters himself that what he has said on this subject alone proves sufficiently that he has been not only an attentive, but an *extensive*, observer of disease "at the bedside." (See *Comm.* vol. i, *all that follows in immediate connection with the foregoing quotation, from p.* 233—239, *and particularly the following also*, pp. 197—209, 342—343, 184—194, 239—309, 328—335; vol. ii, pp. 294—298, 231—249, &c.)

2. "Again, page 236, our author states," says the reviewer, — "'We think that very few will agree with Dr. Wardrop that,' — "there is usually NO APPEARANCE of the buffy coat in blood removed from persons affected with VIOLENT INFLAMMATIONS UNTIL THE LATTER STAGE of the disease, and AT THE VERY PERIOD WHEN *the further abstraction of blood would be* PERNICIOUS." (1)

"'On the contrary, indeed,' Dr. Paine says, 'we find in ninety-nine of one hundred SUCH cases, that the BUFFY COAT is presented at the *first bleeding;* and has *disappeared, more or less, when* the 'further abstraction of blood would be pernicious.'" (*Rev.* p. 398. *Comm.* vol. i, p. 236.)

The author has no comments to make.

3. "'Again, Dr. Wardrop says,' — "*in almost every case* where venesection is necessary, there is present along with the disturbed action of the arterial system some LOCAL PAIN, more or less severe." "'Now this,' says our author, 'is notoriously NOT THE CASE in very many instances of *venous congestion*, in many *chronic* inflammations, and often in severe cases of *pneumonia;* in all of which bloodletting may be indispensable." (*Rev.* p. 398. *Comm.* vol. i, p. 237.)

The author is content.

4. "Dr. Arnott states," says the author, "'that it is a great modern improvement in the practice of the healing art, in bleeding for the cure of inflammation, to take the blood away AS QUICKLY AS POSSIBLE; since INTENSE INFLAMMATIONS of the BRAIN, LUNGS, BOWELS, &c. are EQUALLY removed BY FAINTNESS, WHETHER it happens after the loss of TEN ounces of blood, or of FIFTY.'" "'This,' says our author, 'is a fallacy.'" (*Review*, p. 398. *Comm.* vol. i, p. 215.)

; "'*or even, as sometimes occurs, when it happens without bleeding at all, after merely tying the arm in preparation.*'" (2) (*Comm. ibid.*)

The foregoing words in *Italics* are a part of the author's extract from Dr. Arnott, and are continuous with the word "fifty," where the reviewer broke off. As the reviewer, also, was exhibiting examples of the author's "*style* and *comments*," as well as "practical information," he should have begun the quotation *with* the "comment" as it occurs in the Commentaries. Thus:—"FROM the FOREGOING CONSIDERATIONS will appear the fallacy of the statement made by Dr. Arnott, 'that it,'" etc. The author has no such abruptness, as, "this is a fallacy." The reviewer goes on,—

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(1) "'Wardrop on Bloodletting, p. 41.'" (2) "Arnott's Elements of Physics, vol. i, pp. 470, 472."

"Dr. Paine adds, in a significant note,—'Hence, too, appears the fallacy of applying the *elements of physics* to considerations of this nature.'" (*Review*, p. 398.)

Such is the author's doctrine *throughout* the "Commentaries."

5. "In the same ex cathedra STYLE Dr. Paine criticises the opinions of other writers. He thus concludes his strictures on Dr. Marshall Hall.—'But the most exceptionable part of Dr. Hall's rules, *as it appears to us*, applies to the repetition of bloodletting. "If MUCH BLOOD has flowed," says, Dr. Hall, "before incipient syncope has been induced, revisit your patient *soon*; and you will probably have to REPEAT the bloodletting in consequence of the severity of the disease, especially if you were not called in early in the first instance. If, on the contrary, LITTLE blood has flowed, NEITHER DOES THE DISEASE REQUIRE, nor would the patient BEAR, farther general depletion. Is not this an interesting and important piece of information?"—(*Review*, p. 398.—*Comm.* vol. i, p. 218.)

"We have made these quotations, which we might extend to ANY LENGTH, partly to exhibit the STYLE of Dr. Paine's criticisms, and partly to give greater currency to the valuable suggestions which he so strongly reprobates." (*Review*, p. 398.)

But again, the reviewer, after some general comments:—

6. "In Vol. I, page 337, Dr. Paine says,—'we hold it (bloodletting) TO BE more important in infancy, under equal circumstances, than at any other age; and this ratio increases as we ascend to the hour of birth.' In page 361, among the aphorisms ["general results"] with which he concludes his essay is the following.

7. "'Bloodletting is equally safe at all periods of life, BUT is *most indispensable* in old age.'

"Now, we submit, that if Dr. Paine's practice had been GUIDED BY EXPERIENCE instead of THEORY, he would not have come to conclusions, and have made UNQUALIFIED ASSERTIONS so fraught with danger." (*Review*, p. 399.) [The author has quoted all the examples, and uninterruptedly.]

In the first place, the reviewer will admit that there is a great difference between the expression "*we hold it to be*" and "we hold *that it may be*," which last are the words of the author at the beginning of the quotation marked 6; and this, more especially as the author was then engaged in surrounding the operation of bloodletting in infancy with a variety of precautions. The reviewer will also take notice that he has committed a great injustice by leaving out a part of the sentence which forms the quotation 7, and by inserting the word "but," and by then placing the words "*most indispensable*" in Italics;—by which he makes his author *contradict* himself as to what he had said, in No. 6, of the *greater importance*, in a general sense, of bloodletting in *infancy ;* and this is farther enforced by the management of the quotation 6. The author will now state the "general result" as it stands in the "Commentaries :"—

"It [bloodletting] is equally safe at all periods of life, is most indispensable in old age, though not less important in MANY diseases of infancy." (*Comm.* Vol. i, p. 361.)

The reviewer will farther perceive that he had overlooked *much* of the author's extended remarks on bloodletting in infancy, when he attributes to the author "*unqualified assertions* so fraught with danger." Nor are they "assertions," but the result of large experience, of a long process of reasoning upon that experience, and sustained by numerous citations from the highest authorities in medicine. The author has quoted, in his remarks upon bloodletting in *infancy*, the *language* of *Piorry*, *Sydenham*, *Rush*, *G. Baillou*, *Forestus*, *Evanson and Maunsel*, *Lommius*, and has referred to many others, who go to the full extent with the author as to bloodletting in infancy. In the author's remarks upon *old age*, he first cites Celsus, Hippocrates, Galen, and Trallian, as commending bloodletting in the diseases for which the author has advised it. He also quotes *Wepfer*, *Forestus*, *F. Hoffmann*, *Van Sweiten*, *Vitel*, *Foucart*, *Sir G. Blane*, *Rush*, *Hosack*, *Hourman* and *Dechambre*, *Piorry*, *Frank*, *Gui Patin*, *Fréteau*, *Guersent*, *Lancisi*, *Coschwiz*, and refers to others, as going to the full extent with himself, and even *much farther*, in respect to bloodletting in *old age*.

But, the reviewer farther committed a great oversight in attributing to Dr. Paine, exclusively, the quotations marked 6 and 7, since the author, (finding them justified by his own experience, and by the best of the profession,) *adopted both of them from Dr. Rush*, who goes, however, a little farther than the author. Thus, the "Commentaries" say,—

"Dr. Rush thinks, [says] '*that bloodletting* IS *more necessary in the diseases of* INFANTS, *under equal circumstances, than in adults.*' He was an unhesitating advocate of bloodletting in inflammatory affections at all stages of infancy." (1) (*Comm.* vol. i, p. 336.)

And again Dr. Rush, as to *old age :*—

"It is manifest, from what was stated of Rush's experience of bloodletting in infancy, that he considered the remedy most important at the extremes of life ; for, in another work he says,—'*Experience proves that bloodletting* IS *more necessary, under equal circumstances, in* OLD AGE, *than in any other.*" (2) (*Comm.* vol. i, p. 340.)

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(1) "Rush's Sydenham, p. 167, *note*, and his Medical Observations."

(2) "Rush's Cleghorn's Diseases of Minorca, c. 6, p. 166, *note*."

The author's reviewer, in the BRITISH AND FOREIGN, also quotes the extract 7, without the slightest reference to the author's qualifications of the remedy, or to his numerous authorities, whom he brought to his support.—(See *Reply*, p. 50—51.)

And yet the reviewer was quoting from these pages, and says that he had given to the work "a careful perusal." (P. 393.)

As to the illustrious Dr. Rush, he laid the foregoing "aphorisms" before the world without the *slightest* qualifications; and, indeed, they were so irresistibly prompted by *his* vast "experience," that he appended them as *notes* to the works of others, and where his *notes* are *rare* and *brief*. At another time, in speaking of bloodletting in old age, he says, —

"I have NOTHING TO SAY upon the acute diseases of *old people* EXCEPT to *recommend bleeding* in those of them which are attended with *plethora*, and an inflammatory action in the pulse."(¹) (*Comm.* vol. i, p. 340.)

Equally reluctant is the author to record an observation which immediately follows the foregoing citations from the reviewer. Thus:—

> "The same RECKLESSNESS in recommending bloodletting PERVADES the work." (*Review*, p. 399.)

The author has no farther comment to make, than to refer the reader to the *quietus* he has given this charge at page 50—51 of this *Reply*. But, was there ever a Brunonian who would tolerate a page of the "Commentaries"?

It will have been thus seen, that both reviewers have thought that an *onslaught* upon the author's *practical* habits and information would be the most successful mode of wounding his reputation, as a writer and practitioner. But, the author, having quoted all the evidence to the intended effect, is fully willing to rest his reputation for experience upon the citations, without exception, even in their isolated and mutilated shape.

True, the reviewer charges that the author "not unfrequently perverts the opinions of writers, and even distorts their facts, if they militate against his preconceived views." (*Rev.* p. 400.) But, if such were really the fact, it would have been an easy matter for the reviewer to have shown it, since the author has always referred to the *page* of the work from which he may have quoted, or have derived an opinion, — and this not a little to the surprise of the reviewer. (*Reply*, p. 73.) This course would have been just to the injured, and highly gratifying to the author; since he would have been as ready to *acknowledge* any act of injustice, as he ever will be to defend himself. The reviewer then goes on immediately thus:

> "On the other hand, he arrays on his side of the question [solidism] as solidists or vitalists, men whose writings cannot be so fairly interpreted; and many living writers we apprehend, will be surprised to find themselves in such company.

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(1) "Med. Inquiries and Observations, vol. i, p. 453."

> "For INSTANCE, he designates this Journal as 'that *stable solidist*, the Medico-Chirurgical Review;' and adds 'we consider Dr. Johnson HIMSELF in all respects a solidist.' Now we need only appeal to our readers, whether this Journal has not INVARIABLY ADVOCATED THE OPPOSITE DOCTRINE." "EX UNO DISCE OMNES!" (*Rev.* p. 400.)

But, why has not the reviewer mentioned others? For the plain reason, that he would have incurred the risk of the author's contradiction. It was even an act of rashness to have gone as far he has, as the author will now show. "*Ex uno disce omnes!!!*"

Had the reviewer quoted the sentence from which he has culled the words "stable solidist," &c. there would have been no necessity for the subsequent appeal to his readers, who would have seen the true import of the word "*stable*." Thus:

> "'It is not improbable,' says that *stable* solidist, the Medico-Chirurgical Review, 'that a PRACTICAL BASIS will be laid, for the distinction of fevers DEPENDENT ON THE STATE OF THE BLOOD, from those where the nervous system is the primary seat of disease.'" "'M. Bouillaud is too sharp-sighted not to perceive the weak points of his pet doctrine, and not to see the necessity of admitting that THE FLUIDS as well as the solids may become PRIMARILY ALTERED in some diseases.'" "'*Like most men of sense and experience*, Dr. Maitland inclines to the opinion that *solidism* went as far in one extreme of error, as the humoral doctrine did in the other, — much farther indeed.'" (*Comm.* vol. i, p. 393.)

Indeed, the entire page 393 is devoted to extracts from the Medico-Chirurgical Review, for the purpose of showing that the Journal had become a humoralist, (as we now see it broadly affirmed by its editor,) and the word "stable" refers to the Journal's former defence of solidism, and its more recent support of humoralism. Upon this page the author quotes to the foregoing effect, from "Vol. XV. p. 354; Vol. XXX. p. 388; Vol. XV. p. 337; Vol. XI. p. 337; and Jan. 1839, p. 69." In other places he has similar quotations from the Journal. Introductory to the foregoing, the author has the following remark:

> "As we purpose 'breaking a lance' with philosophers whom we hold in great reverence, and to whom, as we sincerely think, none can be more indebted than ourselves for sound information on other subjects, it seems to us proper that we should *first pay our respects to the* REVIEWERS. Not that there has been any attempt, on their part, to crowd the doctrine, but
>
> "The name of Cassius honours this CORRUPTION,
> And chastisement doth therefore hide his head.'
>
> "We certainly do not intend to review the reviewers. This, we admit, would be indecorous, and beyond our province, since it is conceded that they possess a 'final jurisdiction.' Besides, we are *single handed*,(¹) and have nothing but facts for our weapons. The

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(¹) It is very rare that the author "arrays on his side" any authorities in a *general* sense. Hunter & Bichat he rather *defends* against the imputed taint of humoralism. (*Comm.* vol. i. pp. 626, 633-636.)

contest, therefore, would be manifestly *unequal;* and being so, we have long since made up our minds —'that discretion is the better part of valour.' 'OBSEQUIUM AMICOS, VERITAS ODIUM PARIT;' 'yet would we reluctantly HAZARD the latter by offering our sentiments on the doctrine laid down BY THEM, did we not consider that all enlightened minds are open to the impression of truth, and that in scientific pursuits, it is not *the man* but *the opinion* which is the subject of disquisition.'" (*Comm.* vol. i, p. 391—392.)

"Careless of censure, nor too fond of fame;
Still pleased to praise, yet not afraid to blame." — *Pope.*

And now, as to the author's *special* exception of Dr. Johnson, senior. The author has always had a great veneration for this philosopher, and has endeavoured to manifest it in his "Commentaries." (Vol. ii, p. 671, &c.) His *only motive* for making the reservation was his firm conviction that this eminent man was mainly, if not wholly, a solidist, and it was done merely as an act of justice. It occurs in a *note* at the page where the author was quoting largely from the Medico-Chirurgical Review in behalf of humoralism. Thus: —

"We consider Dr. Johnson himself, in all respects, a solidist. See Med. Chir. Rev. July, 1836, p. 148." (*Comm.* vol. i, p. 393.)

Let us now turn to that number of the Journal, and see how far the author is sustained in this act of common honesty. We read as follows; Dr. Johnson speaking in *propria persona:* —

"There is, after all, but little real difference between DR. ADDISON and DR. JOHNSON. The chief difference is this: The *former* thinks the entrance of *medicines*, as well as poisons, into the circulation, unnecessary. The *latter* thinks that medicines, and poisons in medicinal doses, do enter the circulation before their full remedial agency can be effected, — always excepting, of course, those medicines which act, and are intended to act, *locally*, as purgatives, &c. PROPOSED MODUS OPERANDI OF MEDICINES. I submit that, in cases where medicines are exhibited in medical doses, viz. for the remedy of disease, and not for the destruction of life, the MODUS AGENDI consists of three distinct and consecutive processes, as far as the evidence of our senses is concerned.

"*First.* The physiological action of the medicine itself on the NERVOUS SYSTEM of the part or parts *to which it is applied.*

"*Secondly.* The physiological action or reaction OF THE NERVES on the *vascular*, *glandular*, or *fibrous* tissues of the same part or parts.

"*Thirdly.* The Physiological action *induced* in the *vascular*, *glandular*, and *fibrous* part or parts, by the TWO PRECEDING PROCESSES; in *other words*, the remedial *effects* or *products* of the medicine employed." "In these three consecutive processes, then, this *tria juncta in uno*, WE HAVE THE MODUS OPERANDI OF MEDICINES. The medicinal agency is the first, — the nervous agency is the second, — the vascular agency is the third." "The only question that remains, and in this I differ from the exclusive sympathetic doctrine, is this: — is the nervous system of the various parts *remote* from the seat of *primary* impression, (the stomach, for instance, when mercury is exhibited,) excited into action by purely nervous sympathy with that primary part, or by the presence of the mercury, however decomposed or modified, carried to these parts by the circulation? This is the problem to be solved, and my own opinion is, that the probability is in favour of the latter supposition, — namely, that the mercury permeates throughout

> the whole system, and acts on EACH INDIVIDUAL ORGAN AND PART, THROUGH THE NERVES AND VESSELS, IN THE SAME WAY as it did on THE PART TO WHICH IT WAS FIRST APPLIED." (DR. JOHNSON in *Med. Chir. Rev.* July, 1836, p. 147—149. *Author's* caps. &c.)

Dr. Johnson, therefore, is, or was in 1836, an *exclusive solidist*, though he differs from the author in not *denying* the absorption of mercury and such medicinal agents as do not act as cathartics, or whose action is local. What is true of medicines, is equally so of morbific agents, as to their *modus operandi;* but, we have in the "Commentaries" Dr. Johnson's direct opinion, to the foregoing effect, as to the latter. (See *Comm.* vol. i, pp. 473, 474, 486, &c. Also, pp. 515, 527—529, 546—562.) The author laments the necessity of so much detail; but, this work must not be imperfectly done. The reviewer has no other charge, well or ill-founded, under his remarks on the Humoral Pathology, unless it be that he states that the author

> "Exposes, and attempts to controvert the opinions of all writers, from Galen downwards, who advocate the humoral pathology." Also, — "He is a *confirmed* solidist, and DEMOLISHES, with unsparing criticism, hosts of authors WHOM HE QUOTES, whose views militate against his own." (*Review*, p. 400.)

The reviewer quotes the author as to the great question between the solidists and humoralists, and says, "it is fairly stated by the author." The statement occurs in this *Reply*, p. 58. The reviewer then adds, that "we are satisfied with the answer of M. Andral to this question," which the reviewer quotes, as it occurs in this *Reply*, p. 58, beginning with "Philosophy leads us," etc. (See *Reply*, p. 57, *note*, *Andral;* and *Comm.* vol. i, p. 626–632.)

The reviewer agrees to the "existence of vital forces, properties, or something inherent in living beings *totally distinct* from chemistry, galvanism, or other analogous powers, and which can neither be produced nor imitated by any of these agencies;" but thinks that "many of the processes of life may be modified by chemical and mechanical causes." He objects, however, that,

> "The author treats with great severity the opinions of those philosophers who attempt to explain the phenomena of life on chemical or other analogous principles, and whom he terms the chemical physiologists." (*Review*, p. 394.)

He remarks, in this place, that the author "unfairly" "*demolishes the doctrines of Prout*, *Philip*, *Davy*, *Bostock*, *Elliotson*, *Müller*, *et id omne genus*," (p. 394;) but he does not say in *what instance* any unfairness occurs. The author, therefore, can make no reparation, nor is any one protected by asseverations of so universal a nature. May it not be, also, that there is as little ground for this imputation, as there was for the charge in respect to "the stable solidist" and "Dr. Johnson"? (See *Reply*, p. 78.) As to the style and manner in which the author's criticisms are executed, the world must judge between the author and the few who may not receive the author's remarks in that spirit of liberality which is the general characteristic of philosophical minds. The author certainly made no distinction between the distinguished Journals who are the subjects of these remarks, and those writers who do not exercise the critical pen; and even Dr. Forbes' "Cyclopædia of Practical Medicine" came under the author's consideration. (*Comm.* vol. i, pp. 531—539, 576—581.)

In respect to the Essay on animal Heat, the reviewer states it as the opinion of the author, that,

> "Heat is secreted or eliminated through the united agencies of these [vital] powers from the blood, in the same manner as the bile or gastric juice. This is certainly a very plain and simple statement of the matter. But, there is no great novelty in it. Hunter maintained the SAME OPINION." (*Review*, p. 401.)

Certainly, and so did Bichat, and many others whom the author has quoted to the same effect. Why, then, this *insinuation!* Had not the doctrine become "obsolete" (as *Dr. Carpenter's* reviewer more than implies of the "vital principle,") (¹) when the author wrote? No matter what denials may now be made;—the records are strewed around us. The reader, who may take the trouble of comparing the foregoing statement of the present reviewer with his of the British and Foreign, (*Reply*, p. 60—61) will find an amusing discrepancy between the critics, as on many other topics. He of the Medico-Chirurgical also quotes the remark of Hunter which occurs at that page, (and which, by the way, was printed before the author knew of the existence of the present review), — he quotes it, we say, for the purpose of showing that the theory *belongs* to Hunter, and in a way that leads to the belief that the fact had been overlooked by the author; when, indeed, that quotation, among several others of similar import, stands as a motto to the author's Essay upon this subject. (See *Comm.* vol. ii, p. 1.) Nevertheless,—

> "The author shows clearly, we think," says the reviewer, "that there are *many anomalies* that cannot be accounted for on THE THEORY that animal heat results from RESPIRATION, and the changes induced on the blood by that process." (P. 401.)

The reviewer, with all his skepticism upon the question of solidism, and the author's doctrine as to the physiological influences of bloodletting, acknowledges "a vital principle"; but the author can discern no other reason for it than what he has already assigned at page 44, this *Reply*.

> "But, as this vital principle," says the reviewer, "USUALLY MAKES USE OF CHEMICAL and mechanical agents for its purposes, we may, by investigating the

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(1) "It excited our surprise," he says "when reading the Physiology of Muller, to find him CLINGING to a NOTION which it is HIGH TIME should become OBSOLETE. His 'ORGANIC FORCE' corresponds in most respects to the 'vital principle.'"—*British and Foreign Medical Review, Jan.* 1839, p. 172. See *Reply*, pp. 15, 32—34, 40, 63.

laws of these agencies, be assisted in explaining some of the phenomena of animal heat," &c. (*Rev.* p. 401.) — See *Reply*, pp. 32—34, 40.

The "*chemical* and mechanical agents," therefore, do the work. (See *Reply*, p. 42, *Newton's aphorism.*)

"Coming events cast their shadows before." The reviewer is evidently impressed with the belief that the author has, also, done some service in his Essay on the Philosophy of Digestion.

"In the next," says the reviewer, on "the Philosophy of Digestion, the author states that the GASTRIC JUICE is a substance, sui generis, ENDOWED WITH VITAL POWERS — that it can ONLY BE GENERATED by a LIVING STOMACH — that it CANNOT BE IMITATED BY ART; and that through its agency alone digestion is performed. These are TRUISMS WHICH FEW, in the present state of our knowledge, will be HARDY ENOUGH TO CONTROVERT." (*Rev.* p. 401.)

And this, in the very face of his own chemical theory of organic results, and of the multitudinous, living writers, whom the author has quoted in direct contradiction of all the foregoing propositions. (See, also, this *Reply*, p. 63, *Dr. Carpenter* and *note* 2.) The review, however, has the following *saving* clause in a note,

"For fear some prudish readers should grow skittish."

"For some interesting experiments on ARTIFICIAL DIGESTION, we beg to refer our readers to two Essays by Professor Müller and Dr. Schwann, in 'Archiv. für Anat. und Physiol. for 1836.'" (*Rev.* p. 401, *note.*) [See, also, the same, extensively, in the author's Essay.]

The reader will have also observed that the reviewer goes with the author in supposing the gastric juice to be "endowed with *vital* powers"; or, in the peculiar language of the British and Foreign, he "mixes them up with the gastric juice." (*Reply*, p. 63.)

Having now got rid of vitalism and solidism, (*Reply*, p. 64,) the remaining Essays are disposed of in the following manner: —

"The succeeding Essay is a review of the various theories of Inflammation. All are rejected by our author; and his own, the *vital* theory, considered the only true one.

"The next Essay is on The Philosophy of Venous Congestion. It is most elaborate; occupying more than half the volume. As it CONSISTS, *however*, MAINLY OF QUOTATIONS and CRITICISMS, we shall not attempt to analyze or abridge it. [May it not "consist mainly of" something more?] (*Reply*, p. 64—65.)

["One hates an author that's ALL author." — *Byron.*]

"Two Essays on the Comparative Merits of the Hippocratic and Anatomical Schools, and on the Principal Writings P. Ch. A. Louis, M. D., conclude the work.

"In the brief sketch we have given of this very voluminous and erudite performance, we have endeavoured to lay before our readers the peculiar views and opinions of the author." [?] "We willingly award to him the merit of multifarious reading and research; and of untiring zeal in the support of his doctrines." (*Rev.* p. 401—402.)

Confessed the truth that could not be concealed,
That fraud might drive his author from the field.

The author now comes to his reviewer's most important misrepresentation. The reviewer, in referring to the author's argument derived from Scripture, in behalf of the specific nature of the vital principle, (*Comm.* vol. i, p. 86—98,) quotes the author as follows: —

"Assuming Scripture as a ground of argument, the author says, —

1. "'It is manifest that man was completed in his structure without life before he became endowed with a soul, and that the act, which created his soul, bestowed also the vital forces. One appears to be as much a new creation, distinct from the forces of dead matter, as the other. When man was already perfected in his structure, he was without life. But, by the act of breathing into his nostrils, his peculiar physical life and his soul were simultaneously created; and such is their COMPANIONSHIP WHILST LIFE CONTINUES, that SOME philosophers have considered them IDENTICAL. And how perfectly in harmony is all this with the exit of man. His soul and the vital forces leave the corporeal frame simultaneously; nor will either be RESTORED but by another act of creative energy.' (*Comm.* vol. i, p. 87.)

"He *then* triumphantly shows, [that is, *four pages farther* on,] that, —

2. "'The vital forces cannot be generated by matter, since upon them organization depends; nor by the forces of physics, since these are perfectly incapable of restoring the structure, or even its elementary composition, after the organized matter is decomposed, or of reanimating the machine after decomposition has begun'"; *whilst, on the other hand, these are the forces which lay waste the structure, and only so, after the signs of the vital forces shall have totally disappeared.*' [The words in *italics* being omitted by the reviewer.] (*Comm.* p. 92.)

"We need scarcely point out," the reviewer goes on, "the incongruity of these passages. In the latter, the author states that the vital forces are the *cause* of organization, whereas in the former it is shown that man was *perfected* in his 'STRUCTURE,' [to which the reviewer adds — *or organization*] *before* he was endowed with the vital forces: nor need we advert to the IRREVERENT EXPRESSION that 'the soul will not be restored, but by another act of creative energy.' We DO NOT MEAN TO INSINUATE [!] that in this and SIMILAR PASSAGES the author DENIES THE IMMORTALITY OF THE SOUL; but his language on this — the creation of man, and other sacred subjects, IS SO VAGUE, and OFTEN SO CONTRADICTORY, that WE FEAR DOUBTS may be engendered in the minds of many of his readers." (*Rev.* p. 395.) The capitals are the author's.

"*Fear not* to lie, 'twill seem a lucky hit:
Care not for feeling, pass your project jest,
And stand a critic, hated yet caressed."

First. Taking the import of the extracts in their isolated state, the reader will at once perceive that the imputed "incongruity" has not a shadow of existence; since in the extract marked 1, the author's argument relates to the CREATION of the soul and vital powers by a *direct Act of God;* whilst in that marked 2, the discussion is intended to demonstrate the *Creative* Act by showing, that after the *Act* of creating the vital powers, the reproduction of *organization* is made to depend upon the integrity of those powers in their established connection with organized structure, and that "they cannot be generated by matter, nor by the forces of physics." No. 2, therefore is, as designed, a full demonstration of the Creative Act of God; whilst it is equally opposed to spontaneous generation. But, the whole argument in this place, extending over many pages, bears directly in opposition to the reviewer's imputation of "incongruity."

The reckless, and *ad captandum* assumption is then put forth, (in immediate connection with the foregoing,) that

> "Discrepancies of the kind to which we allude, may be found IN ALMOST EVERY PAGE OF THIS ESSAY, [Vital Powers.] For instance, in the passages we have quoted above [1 and 2,] the author is COMPELLED to have recourse to Divine agency for the creation of his vital forces; yet, in a note, he says that 'the whole work of creation was MIRACULOUS, and therefore is not connected by any analogies with the subsequent processes of NATURE;' [*where is the* "discrepancy"?] and in page 10, he states: — 'Some able writers have lately appeared, who, admitting that life consists of a certain series of phenomena peculiar to organized matter, and having endeavoured to EXPLODE THE ENTIRE DOCTRINE which regards THE FORCES upon which those phenomena have been supposed to DEPEND, have proceeded so far as to affirm that the Deity himself is the IMMEDIATE cause of all the phenomena of nature. [See extracts from D. Carpenter; *Reply*, pp. 10, 46.] The latter construction has arisen, in part, from the irresistible conviction that actions of all kinds require a certain power for their development. With this class of reasoners it will be difficult to argue, since their doctrine is a matter of faith, and not of reason. There is no common ground betwixt us.'" — (*Rev.* p. 395.) [*Where* again, is the "discrepancy"? Here the review breaks off; but the author will go on.] "We will say, however, that whilst we equally acknowledge the *superintending care* of the Creator, his method of governing the material world consists as perfectly in the agency of certain forces appertaining to matter, as the matter consists of something distinct from the Deity. The existence of both DEPENDS EQUALLY UPON HIS WILL; — that is to say, the Maker of the Universe *having brought* them into existence, it is His will that they shall so continue, and that the forces of matter, like the mind, shall operate after a certain manner, and according to their respective endowments. Any thing *beyond this* WE BELIEVE TO BE SOPHISTRY; and, ALL WRITERS who deny to living matter an 'organic force,' begin to expound the actions of life *through the medium of such a force*, the moment that those actions become the subject of consideration." (*Comm.* vol. i. p. 10. — See *Reply*, pp. 9–12, 26–27, 29, 31–34, 36–38, 40, 46–48.)

Here, then, we have all the "discrepancies" that are imputed to the author, and it may be well supposed that his work was thoroughly *hunted*,

"With mind well skilled to find or forge a fault."

But that there is the *slightest* discrepancy in the foregoing statements, the author does not allow; and, he submits to any just and intelligent critic that they are all perfectly compatible with each other, with reason, and with *Revelation*. Creative Energy and second causes, the author holds to be distinct; inasmuch as One is Self-Existent, and the other a subordinate agent created by the Self-Existent Being.

The author has emphasized the word "COMPELLED," in the extract from the reviewer, to show the *quo animo* in which the critic perverts his author's meaning to carry out the imputation of "discrepancies," as well as of infidelity. If the reader will also consider the author's rebuke of the doctrine of "Spontaneous

Generation," and the approval which that Appendix receives from the reviewer, the employment of the word "COMPELLED", in its foregoing connection, will show in itself the temper of mind under which the entire review was written.

> "'Tis hard to say, if greater want of skill
> Appear in writing, or in judging ill."

The British and Foreign Medical Review, and the Medico-Chirurgical Review, having thus put forth comprehensive but vague charges of conflicting statements in the "Commentaries," and the author having shown that all their examples are perfectly exempt from the imputed fault, he will now challenge those Journals to *substantiate* ONE *instance in the whole work*, where there is any clashing of opinions, of doctrines, or of argument. Should the Journals think it due to propriety to accede to this proposition, the author will then solicit the favour either of admitting, through the same Journals, his mistake, or of defending the statement or statements that may be pronounced contradictory; and, in doing which, he will endeavour to be at least as courteous as his critics.

Secondly. As to the charge of gross infidelity. This must surely have been silenced by the reader, from the obvious import of the word "*restored*," (1) — by which the author clearly means, "*restored*" TO "*the corporeal frame*"; the author thereby avowing his belief not only in the "immortality of the soul," but in the *resurrection of the body*. And who will be so impious as to deny that "an Act of Creative Energy" will be exercised in *raising* the dead, and in *reuniting* the soul to the body? Finally, the whole spirit and language of the Essay on the Vital Powers, and of the Appendix on Spontaneous Generation, go to prove the soul's immortality. Of this Appendix, the reviewer observes, that, it "*contains some sensible and well written remarks in opposition to the theory of spontaneous generation.*" (P. 401.) As to the reviewer's general imputation of "similar passages," "vague and contradictory," the author can only commit himself and his reviewer to the ordeal of a juxta-position with the work which is thus impugned; ("TRIO JUNCTI IN UNO";) the author being, of course, perfectly disposed to abide by his own doctrine as expressed in this Reply, at page 43—44.

The author, in speaking of the "*extinction*" of the vital powers, (2) (Reply, p. 19,) would have the reader understand, that throughout his Essay, he contradistinguishes them entirely from the soul, as he also does the "*instinct* of animals," with which they are confounded by Müller and many others. Take the following expostulation, for instance, which is also one of the passages in which the author solemnly avows his belief in the "immortality of the soul," and which stands on the page *opposite to that* from which his reviewer was quoting for the purpose of raising an odious suspicion of infidelity, and who, indeed, subsequently makes the last words of the paragraph apparently the author's sole ground of imputation of materialism against Müller; ("he accuses Müller of materialism." P. 396.) The author, therefore, accomplishes a double purpose in presenting the paragraph; but he will first say, as it respects the German philosopher, that all the premises, from which the following inductions are made, had just antecedently occupied near a page; and, again, other quotations from Müller, to *substantiate* the imputation, are continued in a long note proceeding from the paragraph which the author will now introduce:—

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(1) "RESTORE. L. *Restauro.* To return to a person; to *replace;* to *return*, as a person or thing to a former place; to *bring back.*" (WEBSTER'S *Dictionary.*)

(2) The *vital powers*, the author supposes, will be *recreated* in common with the "incorruptible body," whatever that body may be. The soul never dies.

"Our author's (Müller's) argument, as it respects man and the upper ranks, is founded mainly upon the phenomena of generation; and, although it may be that our author would not advocate materialism, in its proper acceptation, yet when he comes to the *divisibility of the soul* of man 'in a certain limited sense,' and associates it, as a *parallel* case, with the *complete divisibility* of instinct, (or, as our author calls it, '*the mental principle*',) in the lowest animals, it appears to us but little better than materialism. This grows, in part, out of our confounding together mind and instinct; in part from IDENTIFYING BOTH WITH THE VITAL PRINCIPLE; and, in part, from speculating upon a subject so far beyond all human comprehension as the endowment of the FŒTUS with a rational, immaterial, IMMORTAL soul." (1) (*Comm.* vol. i, p. 85–86.)

So emphatic, indeed, is the author's whole argument in support of all the attributes that have ever been ascribed to the *soul*, he was apprehensive, from having regarded the *instinctive principle* of animals as an *immaterial* substance, that it might be thought that his doctrine in relation to the *soul* inculcated the inference that the *instinctive principle* is "immortal" also. He therefore carefully contradistinguished the two; and, finally introduced the following remark, which, in itself, is sufficiently significant of what the author had been saying as to the *soul* of *man*. Thus:—

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(1) We will now have the reviewer's management as to Müller. Thus:—

"Again" says the reviewer, "he accuses Müller of materialism, 'in part from speculating on a subject so far beyond all human comprehension as the endowment of a fœtus with a rational, immaterial, immortal soul.'—Whilst in page 13, he argues on the same point himself;—

[The author will quote from his own work more extensively than the reviewer, placing in italics what is omitted by the reviewer;]

"*And yet, if we admit the foregoing premises,* [*Bichat's,*] *the conclusion will also follow, that the soul has no existence till the brain begins to receive impressions, and its intellectual operations shall have commenced. According to Bichat's rule, if there be no judgment, reflection, &c., in the perfect fœtus, it is like one born without a head. But, Bichat's position is here indefensible; and* [quotation begins] since, therefore, the fœtus or a new born infant has as much a soul as man, we argue, that if the child sees, hears, tastes, smells, and feels, as soon as it enters the world, the properties on which those functions depend, had a full existence in the fœtal state at the time of birth." (*Rev.* p. 396,— *Comm.* vol. i, p. 13.)

The reader will now be able to understand the difference between Müller's speculations as to the soul and the author's, as well as the proper ground of the author's imputation of "materialism, to Müller." The author desires that the whole of his *note, Comm.* vol. i. p. 86, should be examined; when the reader will be also able to do full justice to the author and his reviewer.

"It may be said, that it is the TENDENCY of the FOREGOING DOCTRINE to ASSIGN AN IMMORTAL spirit to brute animals. We think not; although we cannot doubt that the substance on which instinctive actions depend, is IMMATERIAL." (*Comm.* vol. i, p. 97.)

The author is mainly interested, at present, in placing the foregoing subject in its proper aspect, as he is soon to publish a *defence* of REVELATION, in two volumes, as *objectionable* in point of *size* and of "facts" as his "Commentaries," and which will also "savour of the lamp." (1) This work is designed to embrace an examination of *all the principal facts* which lay at the foundation of Theoretical Geology, and will contain the author's Interpretation of the Narrative of Creation, and of the Deluge; that interpretation being in conformity with the literal structure of the Narratives, and, as the author endeavours to show, with the sternest of geological facts.

"Philosophy, that leaned on Heaven before,
Shrinks to her second cause, and is no more." — *Pope.*

The work will also consider, extensively, the Plutonic and Neptunian theories;

"Such as Creation's dawn beheld, thou rollest now." — *Byron.*

Also, the imputed causes of the coal-formations, which the author, in common with other geologists, considers of vegetable origin; — with other, perhaps exhausted, topics.

"But, 'why then publish'? There are no rewards
Of fame or profit, when the world grows weary.
I ask in turn, — why do you play at cards?
Why drink? Why read? — To make some hour less dreary.
It occupies me to turn back regards
On what I've seen or ponder'd, sad or cheery,
And what I write I cast upon the stream,
To swim, or sink ——————— "

The author is, therefore, a practical geologist: and as this may appear to militate with what he has said of his *professional* habits at page 74, it is proper that he should state, that geology and mineralogy have supplied the only relaxation in which he has indulged for more than twenty years. These personal statements having been forced upon the author, he will make no other apology. (1)

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(1) See *Reply*, p. 73. — That great and dignified critic, Samuel Johnson, advises authors — "to consider, how they whom publication lays open to the insults of such as their *obscurity secures against reprisals*, may extricate themselves from unexpected encounters." It is obvious that one of the important expedients, in cases of this nature, lies in raising the veil, and surprising the offender. There is but little in the review now under consideration, by which the author can accomplish this desirable purpose. Nevertheless, he submits, whether the following declaration do not involve a responsibility as to an important position of the Journal, that identifies the writer with "HENRY JAMES JOHNSON, Esq"., the junior editor, — and this more especially as some of his tenets are more characteristic of an "Esquire" than of a Doctor of Medicine.

"Now," says the reviewer, "we need only appeal to our readers, whether this Journal has not *invariably* advocated the opposite doctrine?" [the Humoral Pathology.] (*Rev.* p. 400.)

"A critic was of old a glorious name.
* * * * * * *
Conscious of guilt, and fearful of the light,
They lurk enshrouded in the veil of night." — *Churchill.*

The author is not disposed to leave what he has said, in his work, on the subject of "fossil animalcula," without farther defence. The question is interesting to him from the manner in which he has connected it with physiological investigations; and especially so as this is the only part of his Appendix on the Microscope which he is aware of having been attacked. (See *Reply*, p. 6.)

In the first place, with the foregoing exception, the whole of that Appendix relates to *soft* organic substances; and to the investigation of these, by the microscope, his objections are intended to apply. He has therefore said: —

"It will be readily seen, also, that there is a *vast difference* between the *solid* and *soft structures* of the body, and that minute observations may be *perfectly practicable* in the former case, when they would FAIL ENTIRELY in the latter." (*Comm.* vol. i, p. 707.)

The author certainly never doubted that strong resemblances exist between the supposed *fossil* shields and those of *living* animalcula. But, this does not *prove* the supposed coincidence. The question then arises as to its *probability*, and this will depend much upon the absence of contradictory facts.

Chalk, and its imbedded seams and nodules of flint, are said to be composed of these supposed animalcula. Chalk is light, the flint exceedingly compact. Each must have formed *simultaneously*, according to the hypothesis. Now, then, 1st, how was the requisite amount of animalcula crowded into the spaces occupied by the nodules of flint, which are often of great size? 2d. How are the geodes of quartz to be explained? 3d. Why is the flint composed of *siliceous* shields and the chalk of *calcareous?* 4th. Why is the flint disconnected from the chalk? These questions must be answered in a way that shall have some plausibility. It has been imagined that these enigmatical formations of flint were primitively sponges, which is probably often true; and the author has heard it confidently said that Ehrenberg explains their conversion into *siliceous* animalcula upon the principle that the sponges subsisted upon *this particular species*. Other philosophers, as the author knows, sustain this opinion as the only solution that can be offered; but to which the following apparently insuperable objections apply.

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(1) An appeal to the moral philosopher, upon some of the foregoing subjects, may not be inappropriate; and the author will therefore quote the REV. DR. CHANNING upon his side as to the question between himself and the reviewer regarding the resurrection of the dead; as well as to the distinction which the author has made between the "*work of creation*," and "the subsequent processes of nature." (See *Reply*, pp. 85—86.) In speaking of "miracles" and "Creative Energy," Dr. Channing says, —

"If, then, the great purposes of the Universe can best be accomplished by departing from its established laws, these laws will undoubtedly be suspended." "Nature, then, we fear would not have brought back the world to its Creator. And, as to the doctrine of IMMORTALITY, the order of the natural world had little tendency to teach this. The natural world contains no provisions or arrangements for reviving the dead. The researches of science detect no secret processes for restoring the lost powers of life. If man is to live again, he is not to live through any known laws of nature, but by a Power higher than nature; and how then can we be assured of this truth, but by a manifestation of this Power, that is by miraculous agency confirming a future life?" (CHANNING'S Discourse on Revealed Religion, delivered before the University of Cambridge at the Dudleian Lecture, 1821.)

1st. It supposes that the sponges merely digested the soft parts, without having got rid of the siliceous shields.

2d. It supposes that the sponges continued to devour the animalcula, till they became totally converted into their shields.

3d. It supposes that this curious phenomenon must be going forward in *living* sponges. Are there the necessary analogies?

4th. It does not explain why the siliceous shields of the flint do not abound in the surrounding calcareous deposit, the frequent absence of which must be accounted for.

5th. The animalcula of the flint being specifically different from those of the chalk, increases the 4th objection.

6th. The vast disproportion between the density of the flint and that of the chalk is not explained. Pressure will not account for it, since the accumulation being gradually progressive, the great disproportion should not exist.

Unless the questions, therefore, which have been now propounded, be answered in a more plausible manner, the assumed constitution of nodules and veins of flint must be employed as a lever to overthrow the whole hypothesis. Whatever may be the evidence of affinity between the shields of the supposed fossil animalcula and those of living species, that must be sacrificed to the exigencies of physical impossibilities.

Other difficulties might be raised,—such as the necessary supply of organic nutriment (the vegetable remains in chalk being rare,)—the vast disproportion between the supposed animalcular formations and those of visible animals, &c. If the disconnected state of flints be compatible with the hypothesis that they are casts of spongiform zoophites, the interpretation of their metamorphosis as given by Bakewell, (Geology, p. 245,) and others, would be quite satisfactory. Infiltration might then take place to an indefinite extent, whilst the animalcular doctrine necessarily supposes an apparently fatal limitation. The inquiry, however, is certainly the most interesting and magnificent that has yet fallen under the microscope, and none would be more gratified than the author to see the hypothesis substantiated.

Note.—It has been suggested to the author that he is not sufficiently *direct* in his *note* at page 67. To avoid this imputation, he will now say that the *note* is intended to expose a *plagiarism* of an extraordinary nature. The author has also good reasons for believing, (and such as will be apparent to others,) that the CRITIC! who perpetrated the plagiarism, is a *large* contributor to the BRITISH AND FOREIGN MEDICAL REVIEW;

> "Whereby 'tis plain its light and gifts,
> Are all but plagiary shifts."—*Hudibras.*

The critic who stands thus arraigned, having thought it not inconsistent to draw as well upon the reputation of the author of the "Commentaries," the latter will establish the justice of his imputation, by exhibiting a few examples of what the reader will find, on farther comparison, to abound in the surreptitious articles. It will be also observed that the critic has taken a *critic's liberty* with the style of the American orator.

EXTRACTS FROM DR. CHANNING'S "REMARKS ON THE CHARACTER AND WRITINGS OF MILTON."	EXTRACTS FROM THE BRITISH & FOREIGN MEDICAL REVIEW. REMARKS ON HUNTER.
"The wisdom of each age is chiefly a derivation from all preceding ages, not excepting the most ancient, just as a noble stream, through its whole extent and in its widest overflowings, still holds communication with its infant springs, gushing out perhaps in the depths of distant forests, or on the heights of solitary mountains. We mean not, that Milton should have neglected the labours of his predecessors. We only mean to say, that the stream of religious knowledge is to swell and grow through its whole course, and to receive new contributions from gifted minds in successive generations. We only regret that Milton did not draw more from the deep and full fountains of his own soul. We mean not to complain of Milton for not doing more. He rendered to mankind a far greater service than that of a teacher of an improved THEOLOGY. He taught and exemplified that spirit of intellectual freedom, through which all the great conquests of truth are to be achieved." "We mean not, that MILTON should have neglected the labours of his predecessors." (P. 65.) [Last sentence is the present writer's repetition.]	"The wisdom of every age, WE MAY REMARK, is chiefly a derivation from all preceding ages, not excepting perhaps the most ancient, and should hold communication with them, just as a river, through its whole extent and in its widest overflowings, still continues to receive tribute from its infant springs. [! We wish not, therefore, to acquit Hunter of culpable neglect in this respect; although in his case we regret it the less, because it made him draw more deeply from the full fountains of his own vast and comprehensive mind. By his researches into the book of nature, he rendered to HIS PROFESSION a far greater service than if he had moulded into new and more beauteous forms the wisdom and the wealth of all his predecessors. He taught and exemplified that spirit of intellectual energy through which all the great conquests of truth have been and are to be achieved." "It has ALSO been made a matter of complaint that HUNTER should have been so little acquainted with the labours of HIS predecessors. We have been induced to make these general remarks to save us the necessity of EXTRACTING LARGELY from A WORK which has been SO LONG KNOWN TO THE PROFESSION"! (APRIL, 1839, p. 422.)
"Far from regarding MILTON as standing alone and unapproachable, we believe that he is an illustration of what all who are true to their nature will become in the progress of their being; and we have held him forth, not to excite an ineffectual admiration, but to stir up our own and others' breasts to an exhilarating pursuit of high and evergrowing attainments in intellect and virtue." (P. 66.)	"Far, however, from regarding HUNTER as standing alone and unapproachable, we believe that he is an illustration of what many who dream not of this might arrive at in the course of their being; and we would hold out his fame, not to excite an ineffectual admiration, but to awaken others and OURSELVES [!] to the free use and expansion of OUR noblest faculties." (*Ibid.* p. 423.)
"*The attention to these works has been discouraged* by some objections, on which we shall bestow a few remarks. And first, it is objected to his prose writings, that the style is difficult and obscure, abounding in involutions, transpositions," *and so on.* We mean not to deny that these charges have some grounds; but they seem to us much exaggerated; and when we consider that the difficulties of MILTON'S style have almost sealed up his prose writings, we cannot but lament the fastidiousness and	"On this point WE shall take the liberty of stating OUR OWN sentiments. [!] We cannot admit that the reasons which have been assigned offer a just explanation of Hunter's obscurity of style; first, because they would only account for some inaccuracies of expression, or statements too hastily or insufficiently put together," *and so on.* "While we mean not, therefore, to deny that the charge which has been made has some grounds, we think, at the same time, that it has been much exaggerated; and

effeminacy of modern readers.

We know that simplicity and perspicuity are important qualities of style; but there are vastly nobler and more important ones; such as energy and richness, and in these MILTON is not surpassed. The best style is not that which puts the reader most easily, and in the shortest time, in possession of a writer's naked thoughts; but that which is the truest image of a great intellect, which conveys fully and carries farthest into other souls the conceptions and feelings of a profound and lofty spirit. *To be universally intelligible is not the highest merit.* A great mind cannot, without injurious constraint, shrink itself to the grasp of common passive readers. Its natural movement is free, bold, and majestic; and it ought not to be required to part with these attributes that the multitude may keep pace with it. There are writings which are clear through their shallowness. For ourselves, we love what is called easy reading perhaps too well, especially in our hours of relaxation; but we love too to have our faculties tasked by master-spirits." "Such sentences are worthy and noble manifestations of a great and far-looking mind, which grasps at once vast fields of thought, just as the natural eye takes in at a moment wide prospects of grandeur and beauty. We would not indeed have all compositions of this character. Let abundant provision be made for the common intellect." Impose upon genius no strict laws, for it is its own best law. Let it speak in its own language, in tones which suit its own ear. Let it not lay aside its natural port, or dwarf itself that it may be comprehended by the surrounding multitude. If not understood and relished now, let it place a generous confidence in other ages, and utter oracles which futurity will expound. WE ARE LED TO THESE REMARKS, not merely for MILTON'S JUSTIFICATION, but because our times seemed to demand them. LITERATURE, we fear, is becoming too popular. The WHOLE COMMUNITY is now turned into readers, and in this we heartily rejoice; and we rejoice, too, that so much talent is employed in making knowledge accessible to all. We hail the general diffusion of intelligence as the brightest feature of the present age. But good and evil are never DISJOINED; and one bad consequence of the multitude of readers, is, that men of genius are too anxious to please the multitude, and prefer a present shout of popularity to that less tumultuous, but deeper, more thrilling note of the Trump of Fame, which resounds and grows clearer and louder through all future ages." (*Ibid.* p. 20—22.)

"Without meaning to disparage the 'TREATISE ON CHRISTIAN DOCTRINE,' we may say that it owes very much of the attention which it has excited, to the fame of its author. We value it chiefly as showing us the mind of MILTON on that subject, which, above all others, presses upon MEN OF THOUGHT AND SENSIBILITY. We want to know in what conclusions such a man rested after a life of exten-

when we reflect that the obscurity of HUNTER'S style *has deterred many from availing themselves* of his invaluable labours, we cannot but regret the fastidiousness and effeminacy of modern readers. We are aware that simplicity and perspicuity are essential attributes of a good style; but there are others, as energy and depth of thought, equally noble and important; and in these we will not admit that HUNTER has ever been surpassed. *To be universally intelligible is not the highest merit.* The best style is not that which puts the reader in the shortest time in possession of the author's naked thoughts, but that which is the truest image of a great intellect, and which conveys fully and carries farthest into other minds the conceptions and feelings of a profound and lofty spirit. A great mind, such as HUNTER possessed, cannot, without injurious constraint, lower itself to the grasp of ordinary individuals. Its own natural movement is free, bold, and majestic; and it ought not to be compelled to part with these attributes in order that the multitude may be able to keep pace with it. There are many writings which are clear through their shallowness. For our own part, we prefer easy reading, especially in our moments of relaxation; but we delight, at the same time, in having OUR FACULTIES tasked by master-spirits. There are minds, again, which grasp at once vast fields of thought, just as the eye surveys at once wide fields of grandeur and beauty; and which, in their moments of inspiration, when thick-coming thoughts and images crowd in upon them, pour out their treasures in a manner perplexing to ordinary readers, but kindling to congenial spirits like their own. We would not have all compositions of this character, but WE WOULD IMPOSE no over-strict LAWS on a great mind. WE would let it address us in its own language, and in a tone which accords with its own ear. If not understood at the time, let it look forward with a generous confidence to the improvements of succeeding ages, and utter thoughts which others in future years will unravel. WE HAVE BEEN LED TO THESE REMARKS, not so much IN VINDICATION OF HUNTER as that we think our own times seem to demand them. [!] MEDICAL LITERATURE is becoming in some respects too common and too popular. [!] The WHOLE COMMUNITY is now turned into MEDICAL readers [!] With this we so far acquiesce; nay, we rejoice that so much talent has been employed in making a certain kind of knowledge accessible unto all. We look, indeed, on the general diffusion of knowledge as one of the noblest and most distinguished features by which the coming will be separated from past ages. But good is often CONJOINED with evil; and we fear lest men of genius be led away, by the shout of popular applause or the desire of obtaining *sudden* wealth, [!] from pursuing that *deeper* and stiller *path* where that thrilling note alone is heard which sounds louder and clearer throughout all succeeding generations." (*Ibid.* p. 419—420.)

"Without meaning to disparage the 'TREATISE ON THE BLOOD AND INFLAMMATION,' we may say that it owes very much of the attention which it has excited to the previous fame of its author. We value it CHIEFLY as *showing* us the MIND of a master on a subject, which, above all others, presses itself upon the attention of the PHYSICIAN OR SURGEON. We are desirous of ascertaining in what sive and profound research, of magnanimous efforts for freedom and his country, and of communion with the most gifted minds of his own and former times" "Milton had no dread of accumulating knowledge, lest it should oppress and smother his genius. He was conscious of that within him, which could quicken all knowledge, and wield it with ease and might, which could give freshness to old truths, and harmony to discordant thoughts; which could bind together, by living ties and mysterious affinities, the most remote discoveries, and rear fabrics of glory and beauty from the rude materials which other minds had COLLECTED. Milton had that universality which marks the highest order of intellect. His healthy mind delighted in genius, &c. He understood too well the rights, and dignity, nnd pride of creative imagination," &c. "He had not learned the superficial doctrine of a later day, that poetry flourishes most in an uncultivated soil, and that imagination shapes its brightest visions from the mists of a superstitious age." (*Ibid.* pp. 4, 5.)

"From the very nature of the work, it cannot engage and fix general attention," &c. Milton aims to give us the doctrines of Revelation in its own words. We have them in a phraseology long familiar to us, and we are disappointed; for we expected to see them not in the language of the Bible, but as existing in the mind of Milton, modified by his peculiar intellect and sensibility, combined and embodied with his various knowledge, illustrated by the analogies, brightened by the new lights, and clothed with the associations, with which they were surrounded by this gifted man." (*Ibid.* p. 39.)

"*We prize it* chiefly as a testimony to Milton's profound reverence for the Christian religion, and an assertion of the freedom and rights of the mind. We are obliged to say that the work *throws little new light on the great subjects of which it treats.* Some will say, that this OUGHT NOT TO SURPRISE US; FOR NEW LIGHT IS NOT TO BE LOOKED FOR IN THE DEPARTMENT OF THEOLOGY." "The chief cause of Milton's failure was, that he sought truth too exclusively in the past, and among the dead." (*Ibid.* p. 62.) [*What the critic terms, "relations of ideas;" see Reply, p. 5. Here occur, also, other similar parallels.*]

conclusions such a man rested, after a life of MAGNANIMOUS efforts to unfold the hidden laws of nature, and to trace the order and regularity with which they occur. [*See July No. below.*] The work before us shows that Hunter's natural progress was from one field of discovery to another; that he could give a freshness and vividness to truths which had become worn, we had almost said tarnished, by long and familiar handling; that he could bind together, by mysterious affinities, remote discoveries, and rear fabrics of utility and beauty from the rude materials which other minds had NEGLECTED. [!] Hunter's intellect was naturally creative, restless, stirred by a burning desire for the discovery of truth, and he was conscious of that within him which could quicken all knowledge and wield it with matchless power. In treating of practical subjects, he had not learned the superficial doctrine of later days, of discarding or setting at naught legitimate theory."! (*Ibid.* p. 420.)

"To place it clearly before others, he feels the necessity of viewing it more vividly himself. By attempting to secure his thoughts, and fix them in an enduring form, he finds them vague and unsatisfactory, to a degree which he did not suspect; and toils for a precision and harmony of views, of which he never before felt the need. He places his subject in new lights; submits it to a searching analysis; compares and connects with it his various knowledge; seeks for it new illustrations and analogies; weighs objections; and, through these processes, often arrives at higher truths than he first aimed to illustrate." (*Ibid.* p. 418)

"*We prize*, then, the volume before us, as being the first in which more just views were disseminated, and *a new light thrown over the subjects of which it treats*"! "Hence the reason why so many corrections are required in the new edition, to bring down this portion of the subject in accordance with the more correct views of the present day. This, however, was not the fault of Hunter, but of the age in which he lived. At this we NEED NOT EXPRESS SURPRISE, SEEING THAT THE GROUND HAD BEEN PREVIOUSLY UNTRODDEN." (*Ibid.* pp. 421, 422.) [*A slur upon Hunter's great vital doctrine of inflammation.*]

We shall now repeat Dr. Channing, for the purpose of showing how the plagiarist repeats him three times, — twice in the *April* article, and once in the next following (July) number of the Journal. *See as above.*

"We want to know in what conclusions such a man rested after a life of extensive and profound research, &c. and of communion with the most gifted minds of his own and FORMER TIMES." (Channing's *Remarks on the Character and Writings of John Milton*, Boston, 1830.—See as above.)

[The reader will find in the opposite place, from the July No., a farther repetition of the April No. Dr. Carpenter in his Principles repeats himself verbally; an example of which occurs in this Reply at page 41, — showing the writer's habit.]

"Considering the amount of labour and time spent in this investigation, we are naturally curious to know, even at this DISTANT PERIOD OF TIME, in what conclusions such an acute observer as Hunter rested:" (April, 1839, p. 424.)

"Considering the amount of labour and time spent in this investigation, we are naturally curious to know, even at this DISTANT PERIOD, in what conclusions such an acute observer as Hunter rested." (July, 1839, p. 183. See, also, *Reply*, p. 67, *note* 2.)

The author has endeavoured to present a *variety* in the examples which he has quoted; and, having been induced to be thus explicit, he will carry out the full intention of this note. Throughout his "Examination" of the article in the British & Foreign Medical Review he has identified Dr. Carpenter as the reviewer, and that no doubt should remain upon any mind as to the author's general purpose, he referred in a note at page 67 to numerous parallel passages which occur in Dr. Carpenter's Principles, &c. and the foregoing review of the Writings of John Hunter, in which the reader will detect peculiar views, and identity of language. The author also shows in that note that his reviewer acknowledges the authorship of these articles. Take the following example from the second article on Hunter, relative to the vitality of the blood, the proof of which Dr. Paine's reviewer assumes to himself, — observing, in his article on Hunter, (p. 178), that Hunter had offered "nothing like proof"!!

REVIEW OF HUNTER.	DR. CARPENTER'S PRINCIPLES, &C.
"The strongest evidence that the coagulation of the blood, when drawn from the living body, is the result of the vital properties which it still retains, appears to us to be derived from the fact that a process essentially the same is the preliminary to the organisation of the blood within its natural cavities." "Now, however, it is known that coagulable *lymph* and the *liquor sanguinis* are almost identical, and that the red particles are *passive*, or nearly so, in the coagulation of the blood. There is, therefore, a strong argument from analogy for considering this change in the blood in the same light as in the lymph." "Those who deny vitality to the blood on account of its fluidity, or, in other words, maintain that its properties are only those of inorganic matter, seem to forget that other fluids unquestionably exhibit vital properties." "Will it be maintained that any mixture of chemical products can imitate the effects of these?" "By such comparisons, it appears to us, we may not only refute the objection urged against the doctrine of the vitality of the blood, on the score of its fluidity, but obtain an affirmative argument of no mean value." (*Rev.* of Hunter, July, 1839, pp. 179, 180.)	"The *liquor sanguinis*, or fluid portion of the circulating blood, is that in which the tendency to coagulate exists, and it is probably that which is chiefly concerned in supplying nutriment to the tissues — the globules, so far as can be ascertained, being merely *passive* in the circulation." "It is almost impossible to consider it without admitting that the *liquor sanguinis* is as completely possessed of vitality as any solid tissue of the body." "An organized character is not, however, peculiar to living solids; (see *Reply*, p. 36—42,) for some traces of it may be detected in the circulating fluid, which is also possessed of properties that must be considered as *vital*, since they differ from any which a mere mechanical admixture of the ingredients could present. Thus, the phenomena of the coagulation of the blood cannot be satisfactorily explained without this admission." (CARP. *Princip.* Sec. 304, 343.) [The whole of the "proof of the vitality of the blood," in the articles on Hunter, is repeated hereabouts. Besides the references in *Reply*, p. 67, compare *April* article, 1839, pages 425 to 427, 429 to 435, 440, with CARPENTER'S sections 364, 365, 366, 367, 368, 84, 203, 78, 115, 5; and *July* article, 1839, p. 171 with s. 303; p. 175 with s. 318; p. 174 with s. 367, 368; p. 180 with s. 304; p. 173 with s. 415; p. 177 with s. 365; p. 181 with s. 304; p. 192 with s. 301, 305; p. 186—187 with s. 307; p. 188 with s. 308.]

Again, Paine's reviewer *claims* the authorship of Dr. Carpenter's article on "Vegetable Physiology." Thus:—"But we may take this opportunity of stating that our belief in the general proposition, which we long ago put forth in an interrogative form, Vol. 4th, p. 20, &c.;" (*Rev.* of Paine, p. 398. See this *Reply*, p. 43.) — Now, take the following from DR. CARPENTER'S *Principles*, &c.: —

> "In the following pages is embraced the substance of an Essay on the 'Laws regulating Vital and Physical Phenomena,' to which was adjudged the annual student's prize, &c.; and also of an Essay on Some departments of VEGETABLE PHYSIOLOGY," &c. The author has freely availed himself, also, of the liberal permission of the editors of the British and Foreign Medical Review to make what use he deemed proper of his contributions to that Journal; especially in regard to two papers, — one on the Study of Physiology as an Inductive Science, and the other on the Functions of the Nervous System, — which have been recently honoured with a place in its pages." (*Preface*, p. 7—8.)

Again, Dr. Paine's reviewer acknowledges the authorship of the article on "Macilwain's Medicine and Surgery *One Inductive Science*," (*Rev.* of Paine, p. 400,) and in that article the reviewer says, — "We are not without hope that the exposition which we gave in our *last* number of the objects and means of Physiological enquiry," &c. (*Rev.* of Macilwain, July, 1838, p. 98.) Now, this "last" article is the one on "*Physiology an Inductive Science*," and which Dr. Carpenter acknowledges in his "*Preface*." The acknowledgments are made rather in the way of *claims*; and the coincidences in views and language between the different articles are such as to dissipate all doubt as to the *individuality* of the author. The readers of the Journal will also readily recognise numerous other elaborate articles by the same hand, who appears to be the critic-general for the Journal. The author may safely say that our plagiarist averages more than one very extensive article for the several numbers of the Review, and that most of the principal writers, especially on Physiology and Pathology, whose works have been reviewed in the Journal, have been assigned to his critical pen; and, if the reader will now glance his eye over the articles, he will find in their general spirit a remarkable contrast with that which distinguishes the Review of Carpenter's "Principles," which, with its author, is lauded without mitigation. Of the extent of this hostility to distinguished worth, and of its common source, the present writer had no just appreciation till he began this "Examination." But, the spell is broken, and worth redeemed.

Connected with the foregoing review of Hunter's works are critical remarks upon Macartney on "Inflammation," Carswell's "Illustrations," &c., and Rasori on the "Theory of inflammation." The reader will find in the July No. many parallel criticisms upon the writings of Macartney with those which are bestowed upon Paine's. The remarks upon Macartney's doctrine as to the operation of bloodletting are strikingly peculiar and coincident. (*Rev.* p. 190.) Take, also, the following triple coincidences of peculiar views on the subject of *animal heat*: —

Hunter's, &c. Reviewer.	Paine's Reviewer.	Dr. Carpenter's "Principles," &c.
"Dr. Macartney is more than usually mystical upon the subject. He quotes the experiments of Brodie, as if their fallacy had never been exposed, [!] to show that animal heat does not depend upon respiration; and considers that we are to look upon animal heat as 'one of the phenomena of sensibility.' We do not see how this can be regarded as a step in the inquiry. There can be no doubt that its evolution is greatly influenced by the nervous system; but, as we conceive, only through the medium of those molecular changes to which it is immediately due, and which are without doubt controlled to a certain extent by the agency of that system." "It is a well-known fact, that, when a portion of the surface of the body is inflamed, the respiration (so to speak) of that portion, — in other words, its conversion of the oxygen of the air in contact with it into carbonic acid, — is increased; and this, harmonizing with what we know from other sources of the relation between the disengagement of carbon from the body and the maintenance of its temperature, seems to us nearly decisive of the question"! "We are fully convinced that it is in the altered state of the nutritive processes that we are to look for one cause of the high temperature," &c. (*Rev.* of Hunter, &c. July, 1839, p. 197—198.)	"Now what will our readers think is the doctrine, &c. Neither more nor less than this, that animal heat is a secretion; and, like other secretions, a production of pure vitality." "We believe it (respiration) to operate through the nutritive processes, and the molecular changes which they involve." "In Dr. Paine's opinion, then, as in ours, the acknowledged influence of the nervous system upon the production of animal heat is exercised through the medium of the organic functions." "We consider it as a result of those functions produced by the molecular changes which they involve, — these changes being themselves governed by the ordinary laws of chemistry, *as we have elsewhere endeavoured to show*." "In these processes there are probably many chemical changes involved, besides the combination of carbon and oxygen; and it is to the aggregate of all these, — the last, however, being the chief, — that we attribute the liberation of caloric which was previously latent." "The case stands simply thus: oxygen is introduced into the system from the atmosphere. In some part of the system it combines with carbon, set free in the ordinary processes of nutrition; and in this state it is thrown off in the gaseous form from the body. Now will Dr. Paine tell us why, since we know that the combination of carbon and oxygen elsewhere produces heat, it should *not* do so here?" (*Rev.* of Paine, p. 393 — 396.	"That many of the nutritive processes are connected with the evolution of animal heat can scarcely be doubted; but it seems peculiarly to depend upon those changes in which the function of respiration is concerned, — viz. the extrication of carbon from the system, in combination with oxygen derived from the atmosphere. Wherever the aeration of the blood is extensively and actively carried on, there is a proportionate elevation of temperature." "The carbon which is thus received into the blood is evidently disengaged from the tissues during the process of nutrition, &c. This (the liberation of caloric) will in general be nearly uniform throughout the system; but there are many cases in which increased action is going on, either as a natural condition or as a diseased state; and a higher local temperature is thus produced. This would seem to be in some degree connected with the influence of the nervous system; but it may be regarded as probable that the evolution of caloric is not dependent upon nervous action in any other way than through those organic processes which stand in relation to both. The results obtained by various experimenters would appear to indicate, that some other organic processes, besides those connected with the excretion of carbon through the lungs, must contribute to the maintenance of the heat of warm-blooded animals" (Carpenter's *Principles*, &c. *Sec.* 494.)

In conclusion, the author will state a few parallel passages from Dr. Carpenter's *Principles*, &c. and the Writings of the Rev. Dr. Channing, by which his circle of evidence will be complete. To avoid all unnecessary complexity, as well as to preserve an unity of illustration, the parallels will still be limited to the writings of the *Rev. Doctor*. The reader will have observed, at the close of the foregoing coincidences with Channing, the tact of the CRITIC in disguising his goods, without essentially impairing their lustre.

Dr. Channing's Works.	Dr. Carpenter's Principles.
"The great use which I would make of the principles laid down in this discourse, is to derive from them just and clear views of the nature of religion." "Shall I deem a property in the outward Universe as the highest good, when I may become partaker of the very Mind from which it springs, of the disposing wisdom, through which its order, beauty, and beneficent influences subsist." "We regard nothing so important to a human being, as the knowledge of his own mind, and of its intimate connection with the Infinite Mind." "To bring the created mind into living union with the Infinite Mind, so that it shall respond to Him through its whole being, is the noblest function, which this harmonious and beneficent Universe performs." (Channing's *Works*, pp. 198, 468, 469, 492.) "In proportion as the TRUE and sublime CONCEPTION of God shall unfold itself in the soul, and shall become there a central SUN, shedding its beams on all objects of thought, there will be a want, &c. It will be felt that the poet has seen nature only UNDER CLOUDS, if he have not seen it under this CELESTIAL LIGHT." (*Ibid.* p. 208.) "In proportion as we approach and resemble the Mind of God, we are brought into harmony with the Creation; for, in that proportion we possess the	"Every step which we take in the progress of generalization, increases our admiration of the beauty of the adaptation, and the harmony of the action, of the laws we discover; and it is in this beauty and harmony that the contemplative mind delights to recognise the wisdom and beneficence of the Divine Author of the Universe. This, in fact, is one of the highest results to which the exercise of our intellectual faculties should lead; and we cannot but believe that the Creator, in endowing us with these faculties, intended that they should conduct us nearer to the CONCEPTION of His Infinite Mind." "When our knowledge is sufficiently advanced to comprehend these things, then shall we be led to a far higher and nobler CONCEPTION of the Divine Mind than we have at present the means of forming. But, even then, how infinitely short of the reality will be any view that our limited comprehension can attain, seeing, as we ever must in this life, '*as through a glass, darkly;*' how much will remain to be revealed to us in that glorious future, when the Light of Truth shall burst upon us in UNCLOUDED LUSTRE." "In proportion to our attainment of the generalizations to which we are thus led, we acquire fresh proofs of the Omnipotence of Creative skill; for,

principles from which the Universe sprung, we carry within ourselves the perfections of which its beauty, magnificence, order, benevolent adaptations, and boundless purposes, are the results and manifestations.

It is possible, that the brevity of these hints may expose to the charge of mysticism, what seems to me the calmest and clearest truth. What man can examine the structure of a plant or an animal, and see the adaptation of its parts to each other and to common ends, and not feel, that it is the work of an Intelligence akin to his own, and that he traces these marks of design to the same spiritual energy in which they had their origin?" "Nature in its lowest and inanimate forms, is pervaded by His Power, and when quickened by the mysterious property of life, how wonderfully does it shew forth the perfections of its Author. How much of God may be seen in the structure of a single leaf, which though so frail as to tremble in the wind [and be *torn*,] yet holds connections and living communications with the earth, the air, the clouds, and the distant sun; and through these sympathies with the Universe, is itself a revelation of the Omnipotent Mind." (CHANNING's *Works*, pp. 458, 466.)

"The discoveries of science have continually added strength to that great principle, that the phenomena of the Universe are regulated by general and permanent laws, or that the Author of the Universe exerts His Power according to an established order. Nature, the more it is explored, is found to be uniform. We observe an unbroken succession of causes and effects. Many phenomena once denominated irregular, and ascribed to supernatural agency, are found to be connected with preceding circumstances, as regularly as the most common events. The comet, we learn, observes the same attraction, as the sun and planets. That attention to the powers of nature, which is implied in scientific research, tends to weaken the practical conviction of a Higher Power; and the laws of the Creation, instead of being regarded as the MODES of Divine operation, come insensibly to be considered as fetters on his agency, as too sacred to be suspended even by their Author. When a new phenomenon now occurs, no one thinks it miraculous, but believes, that when better understood, it may be reduced to laws already known, or is an example of a law not yet investigated."

"To a man, whose belief in God is strong and practical, a miracle will appear as possible as any other effect; and the argument against miracles, drawn from the uniformity of nature, will weigh with him, only as far as this uniformity is a pledge and proof of the Creator's disposition to accomplish his purposes by a fixed order or mode of operation. The Creator's regard or attachment to such an order may be inferred from the steadiness with which he observes it; and a strong presumption lies against any violation of it on slight occasions, or for purposes to which the established laws of nature are adequate. God adheres to the order of the Universe because it is most suited to accomplish his purposes." "If, then, the great purposes of the Universe can best be accomplished by departing from its established laws, these laws will undoubtedly be suspended." (CHANNING's *Works*, pp. 335, 339. Boston, 1830.)

at every successive step, are we able to comprehend new relations between facts that previously seemed confused and insulated, new objects for what at first seemed destitute of utility; and in the same proportion will the contemplative spirit be led to appreciate the vastness of that Designing Mind, which, in originally ordaining the laws of the animated world, could produce such harmony and adaptation amongst their innumerable results.

To avoid all chance of being misunderstood in these views, it may not be useless to adduce, in illustration of them, one of the most obvious and simple adaptations every where presented in the structure of animals,—that of the muscles to the skeleton. We constantly find, in pursuing our anatomical inquiries, that for the advantageous attachment of muscles to bones, some particular form of the latter is provided" &c. "But he (the physiologist) will obtain a much more elevated view of the nature of Creative Power, if he carry his inquiries farther." "Those (facts in physiology) which would seem to be of the most trifling consequence, if viewed in this light only, (physiologically,) are often found, when properly applied, to possess an unexpected and momentous import. A broken twig, a *torn* leaf, a flattened blade of grass, are signs which an ordinary traveller would pass without observation; but, to the practised eye of the denizen of the woods, they are alike certain and expressive." (CARPENTER's *Principles*, &c. Sec. 598—601.)

"The unchangeableness of His nature is manifested by his continued action in the material Creation, according to the same plan by which He at first adjusted the relations of its parts. Our belief in the uniformity of Nature, which leads us to seek for a common cause when a number of similar phenomena are presented to our observation, is based, not only upon experience, but upon the conviction which every believer in the existence of the Deity feels of his immutability. If it were otherwise, we should be led by *analogy* only to infer the existence of law and order where none is evident." "To imagine, therefore, that the plan of the Universe, once established with a definite end, could require alteration during the continuance of its existence, is at once to deny the perfection of the Divine attributes; &c. Let it be borne in mind, then, that when a law of Physics or Vitality is mentioned, nothing more is really implied than a simple expression of the MODE in which the Creator is constantly operating on inorganic matter, or on organized structures." "If these (miraculous interpositions) are exceptions to general laws, they are so only in human estimation; since they are as much a part of the Divine Will, and were as much foreseen by Divine Omniscience, as any of those occurrences which are usually regarded as constituting the order of Nature."

"To suppose that the adaptation of these laws to each other, and to those of the external world, could be otherwise than perfect would be to cast a stigma upon Infinite Wisdom." "Thus, the motions of the solar system all result from that universal property of matter, gravitation, which, originally balanced against other forces, will continue to produce the same effects as long as may be consistent with the designs of the Creator." (1) (CARPENTER's *Principles*, &c. Sec. 147, 148, 9. London, 1839.)

[Dr. Channing abounds with eloquent remarks to the foregoing effect, and they are common in the "*Principles*."]

(1) See the contradictions (*Reply*, p. [illegible]—11, [illegible], 29, 31—[illegible], 62, &c.) to which this habit of dependence unavoidably leads; since such a writer can be guided by no "*Principles*," &c.

Errata, continued from page 72 — At p. 67, thirteenth line from bottom, for 332, read 432. Page 76, second line from bottom, for *he had said* read *the reviewer has fabricated*.

NOTICE OF REVIEWS

BY THE

BRITISH AND FOREIGN MEDICAL REVIEW, AND THE MEDICO-CHIRURGICAL REVIEW,

(APRIL, 1841,)

OF THE

MEDICAL & PHYSIOLOGICAL COMMENTARIES,

AS CONTAINED IN THE

BOSTON MED. AND SURG. JOURNAL OF SEPT. 1841,

BY

THE AUTHOR, MARTYN PAINE, M.D. A.M.

PROFESSOR OF THE INSTITUTES OF MEDICINE AND MATERIA MEDICA IN THE UNIVERSITY OF NEW YORK.

* * *

BOSTON:
D. CLAPP, JR. PRINTER......184 WASHINGTON STREET.
1841.

NOTICE OF REVIEWS, &c.

It is my present purpose to show that Dr. Carpenter (whom I have identified as the author of the pretended review of my "Commentaries" in the British and Foreign Medical Review), and the school that maintain the existence of the vital properties in the elements of matter, are necessarily in conflict with Revelation, as with the highest dictates of reason.

"The doctrine," says my reviewer, "which Dr. Carpenter has propounded respecting *vital properties*, and which is essentially the same as that upheld by Dr. Prichard, Dr. Fletcher, Mr. Roberton, and other able writers upon the same side, may be concisely stated as follows:—Certain forms of matter, especially oxygen, hydrogen, carbon and nitrogen, are endowed with properties which do not manifest themselves either in these elements when uncombined, or in those combinations of them which the chemist effects by *ordinary* means. But they do manifest themselves when they are united into those peculiar compounds which are known as organic, and when these compounds have been submitted to the process which is termed organization. We ASSERT, then, that the very act of organization causes the materials acted on to exhibit properties quite distinct from those ordinarily termed physical and chemical, which properties cannot be caused to manifest themselves in any other way than by the series of operations just described. No one *can say* that the properties do not exist in a *dormant* state because they do not manifest themselves to him." "We argue that they [the vital properties] were as much present in the *elements as any of their other properties*, which only exhibit themselves in certain conditions."—*(Review, April*, 1841, pp. 389, 390.—*My Italics, throughout.)*

And thus Dr. Carpenter, in his "Principles of General and Comparative Physiology," who must abide his own principle of analogy.

"It cannot, then, be logically correct, to speak of vital properties as *superadded* to organized matter, although an apparent analogy has been drawn from physical science in support of the assumption." "If an analogy exist between the two processes, which can scarcely be denied, it leads us to the belief, that JUST as the MAGNETIC POWERS are developed in IRON, when the metallic mass is placed in a condition to manifest them, so the very ACT OF ORGANIZATION develops VITAL POWERS in the tissues which IT CONSTRUCTS [!!] For no one can *assert* that there does not exist in *every uncombined particle of matter*, which is capable of being assimilated, the *ability* to exhibit *vital actions*, when placed in the requisite conditions."—*(Carp. Princip.*, p. 137.—1839). The reviewer has the same parallel. Is there the most remote "analogy"?

There occur in my late "Examination" of the foregoing review the following extracts and remarks.

"'But, we take this opportunity,"' says the reviewer, "'of stating that our belief in the general proposition, that 'plants or animals of a high degree of organization are *capable of producing from various parts* of their tissues *beings* corresponding to those of the inferior orders of their kingdoms,' has recently been much strengthened by additional evidence.'" —*(Rev.*, p. 393.) What is the evidence?—See *Comm.*, Vol. 2, p. 130.

"Dr. Carpenter is of the same opinion. Thus:—

"'It appears very difficult, and indeed almost impossible, without some admission of this kind, to account for the production of *parasitic* plants and *animals* in the interior of others. That their *germs* have been conveyed from without into the situations where they are developed, must be held as a very forced supposition,' &c.—*(Carp. Princip.*, p. 395.*)*

"Suppose it so;—is not the organization of the *parasite* as absolutely *specific* as that of the more complete animal—it may be beast, it may be man? Where, then, must this doctrine conduct our philosophers? *Professions*, in such a case, are nothing; and they are nothing when *God* is confounded with *nature*.—(See *Exam.*, p. 10.) We must look at the inevitable consequence of the *principle;* whilst Dr. Carpenter and the reviewer have also laid the broad foundation, that all the *vital properties* there are, exist in the elements of matter, and the former goes so far as to say that—'*We may believe that there exists in all matter a tendency to become organized*' *(Carp. Princip.*, p. 394), and that the *elements may be organized 'by the hand of man'!—(Exam.*, p. 40.) Compare with Tiedemann's doctrine in *Comm.*, Vol. 2, p. 124."—*(Examination*, &c., p. 43.*)*

Doubtless, many will consider my proposition already made out; but there is yet remaining another and *conclusive* demonstration. It is admitted, by our premises, that there is nothing to show the existence of vital properties in the elements of matter, and that they are only manifested when the elements become organized. The right is assumed, however, of maintaining that they do so exist, and that it will not be surrendered till its opponents prove the self-evident absurdity. This postulate, it will be seen in the first place, is subversive of all philosophy, and that La Place, with a far greater show of reason, insisted that the nebular state of the universe, which he supposes *(Exposition du Systéme du Monde, l. 5me, c.* 6), had the fundamental requisite for the Platonic doctrine of creation,—that is to say, a rotation upon its axis, and thus carried out a fascinating system which lays the foundation of the universe in the principle of spontaneity. La Place thus saw the necessity of avowing atheism, which he did without subterfuge, and with a manly responsibility. We need not, therefore, controvert his nebular doctrine. Our next step is the admitted fact that the phenomena of the vital properties are *sui generis*—that they are not manifested by inorganic matter, but are peculiar to organic. According to our opponents, however, this constituted no proof of the non-existence of the vital properties in the elements of matter, and they therefore rest upon the *assumption* of such existence.

But, the manifestations of the *soul* are not more peculiar to man than the phenomena of organic life, and it follows from our premises, by irresistible analogy, that the *soul* must, equally with the vital properties, exist in the elements of matter, and like those properties undergoes development by the organization of the elements; and that our opponents, upon their own ground, must assume this as fact till it can be otherwise demonstrated, and by the same process of inductive philosophy which they require as to the non-existence of the vital properties in the elements of matter. We thus arrive at a proof which no sophistry can invalidate, that the Edinburgh Journal was sound in its conclusion, that Dr. Carpenter has inculcated in his "Principles" the doctrines of infidelity.—(*See Edinburgh Medical and Surgical Journal*, *Jan.*, 1840.) And so, exactly, of my reviewer.

And again, Dr. Carpenter, and my reviewer, maintain that when man dies and is resolved into the elements of matter, his vital properties continue to exist in those elements; and that when these elements become a part of the organization of inferior animals or of plants, his vital properties share the same destiny. It follows, therefore, that the soul must observe the same rule of construction—appearing under the manifestations of instinct in animals, and in plants according to the nature of their organization.

On the contrary, those, who entertain the belief of a Creative Power, and of the immateriality and immortality of the soul, and that it was superadded to man after the creation of his organized structure, as set forth in Revelation, by assuming the truth of this proposition, will find in the foregoing argument a full demonstration that the vital properties must have been equally *superadded* as a distinct creation; since the manifestations of the properties of life are not less various, remarkable, and peculiar to organic beings than those of the soul. The analogy is as good in one case as in the other, and is confirmed by all the evidences of nature. It is a species of analogy, too, rather more to the purposes of philosophy than that which Dr. Carpenter assumes between the development of "magnetic powers" by placing a bar of iron in an erect position, and the development of "vital properties" by the *conversion* of the elements of matter into *organized tissues* (so replete with the highest evidences of design), and then extorting from this extraordinary assumption the conclusion, that the "vital properties" like the "magnetic powers" exist in the elements of matter. Our author cannot, at best, escape from his own logic; and the proof, upon his own ground of a coincident analogy, must be valid, or otherwise, according to the strength of the analogy. The reviewer has exactly the same argument as to the "magnetic powers," and therefore falls under the same category.

Dr. Carpenter, in his defence of himself against the "charges" of the Edinburgh Journal, quotes the opinions of his own school to justify his doctrines. But, why not defend them himself? Why this habitual dependence upon others? Why not take the natural course of the present writer? Those are questions, too, on which something more than the authority of opinion is wanted; nor will Dr. Carpenter longer contend "that to none of their arguments has any formal reply been made." Our author says farther, also, "it will be easy for me, should they [the charges] ever be repeated, to bring forward a body of testimony, which, with those unaccustomed to inquiries of this kind, will weigh more than argument."—(*See British and Foreign Med. Rev. April*, 1840.) Of course, our author will now "bring forward the body of testimony"; but since it is not to possess the merit of "argument," it may be expected that it will not be a reiteration of the opinions of Dr. Prichard, Dr. Fletcher, and Mr. Roberton, which have been already produced by Dr. Carpenter in his "Defence," in his "Principles," and in his review of my "Commentaries."

The late unexampled misrepresentation and injustice with which I have been treated by the British and Foreign, and Medico-Chirurgical Reviews, and which I have exposed in a pamphlet rather than to avail myself of the courtesy of the American medical press, appear to my mind to justify the addition to this communication of a few extracts from my "Commentaries," touching the utterly unfounded charge of a disbelief in the *immortality* of the soul, preferred, apparently, by the junior editor of the Medico-Chirurgical Review, and whose very mode of misrepresenting my faith upon this question convicts him of the alleged infidelity, as it does of the most unaccountable disregard of truth. And yet I would not that this statement should imply that I am annoyed, since it is made as an inevitable dictate of truth, and for the purpose of its advancement. I would, also, farther premise, that no little part of my Essay on the Vital Powers is devoted to a proof of the *immateriality* and *immortality* of the soul, as connected with my demonstration of the specific existence of the *vital principle*. An exemplification of this fact is exhibited in my "Examination," &c., and I shall now subjoin an example which illustrates the whole object of my "Appendix on Spontaneous Generation," as it does of the religious tone which is infused into the whole work. I have no apprehension that justice will not ultimately come, and in all the measure that I can desire from my cotemporaries; but I am, nevertheless, disposed to anticipate the slow march of truth, and to test, upon the foregoing question at least, *and on my own native soil*, a principle which has been attributed to man at all ages as a proof of his moral obliquity.

Having gone over, in my "Appendix on Spontaneous Generation," with my physiological evidence against this doctrine, and that of *materialism*, I have many remarks of the following import.

"The manifestations of mind, by admission, appertain to the brain, nor can any other part of the body produce a single act of intellection. But, the brain enjoys, also, in the highest degree, the powers and functions that belong to other complex organs,—has its circulation, nutrition, secretion, and presides, more or less, over the organic functions of other viscera. All these are manifestly *organic* functions, which have their analogies in various other parts. There is *something*, however, *superadded* to this organ, to which there is nothing analogous in the rest of organized matter; whilst all other organs have the plainest analogies in their several functions. It is clear, therefore, that the phenomena of mind are the result of the *combined action* of this *something* (which rational philosophers call the *soul*) and the material part. The same arguments which are employed in another place (Essay on the Vital Powers) to show that the powers of life are *something*, and not a mere matter of fancy, are equally applicable for demonstrating the real existence of the *soul* as contradistinguished from *nothing*; and we think the proof is the same, and as palpable, in one case as in the other." [It will be seen that I have arrived at the same result in this article by a new process of induction, both as it respects the soul and the vital principle. Other new methods appear in my "Examination," &c., pp. 33, 39, 40. Can they be set aside?]

"Although we are disposed to give a liberal construction to the Holy Scriptures, we think there should be no violation of any direct statements which they make, however they may appear incapable of explanation, or adverse to the researches, or the learning, the philosophy, or the ambition of man. In our investigation of the works of nature, it should ever be a primary object to render our discoveries subservient to the Revelation which respects creation, and the extension of true philosophy will surely follow. And, should we now and then meet with apparent obscurities, they should be rather regarded as tending to establish our general position, since it is God alone who is the Author of mysteries; and whenever they have been clearly expounded, they have always appeared consistent with whatever had been known of His Providence, and the most obvious import of Revelation. 'It is the glory of God,' says Bacon, 'to conceal a thing, and the glory of the king to find it out.' But, above all does it behoove the geologist, the physician, the chemist, and all others who are employed in the investigation and interpretation of nature, to be faithful to the lofty trust which is committed to their care. They should be cautious of breaking up the great chain of creation, and of reducing the noble parts to the most ignoble. Least of all can any philosophy endure which is opposed to the fundamental acts of creation, because it would not then be founded upon nature. Whoever, therefore, may be an unbeliever, will find it for his interest as a philosopher, to admit the Attributes of a Creative Power. We are fully sensible, however, that, in the ardor to account satisfactorily for anomalous events, we may unintentionally misinterpret the established order of nature," &c.

"The discussion with which we began this Appendix naturally conducted us to that of '*materialism*.' The subjects being intrinsically of a popular nature, we may, for a moment, descend from the altar of science and approach the precincts of the pulpit. This we do for the purpose of saying that physiology should become an element in the education of clergymen. The enemy of religion, or the well meaning but mistaken cosmographer, takes advantage of your want of familiarity with this department of knowledge. They tell you that the living system has no forces peculiar to itself, and that it is wholly amenable to such as rule in the inorganic world; and they conduct you at last, by these premises, to an almost irresistible admission that living beings may be created by their power. And we have already shown you, when thus prepared, how easy a matter it is to spread before you, without greatly shocking the religious sense, a plan of creation which ascribes the origin of animals to 'spontaneous generation,' as it is called in preference to 'chance.'

"The progress towards infidelity is always slow,—at least apparently so in a Christian land; and, whenever the consummation may take place, regard for reputation, and a more successful propagation of the doctrine, will surround it with reservations, insinuating analogies, and perhaps with some show of religion, either for the affected purpose of impartiality, or to furnish a loop-hole of retreat, should the enemy crowd hard. The steps are gradual from the incipient errors in natural philosophy to a disbelief in the Mosaic record of creation. When we have ultimately reached this brink of the precipice, there is but one dreadful plunge, and we are then in the vortex of atheism. We may begin, as we have said, with a simple denial of the living powers of organized beings, and it will become, at last, an easy argument upon this, and analogous premises, that the Almighty had but very little, if any agency, in the most sublime part of existences. But, when you shall look at physiology in its true aspect, you will see that the living, organized kingdoms are governed by laws totally different from any thing that is known of the inorganic. This will assure you that there can be no 'spontaneous generation,'—that the forces of physics can have had no lot in the creation or in the perpetuity of animals; but, on the contrary, it is their work to *lay waste the whole fabric of creation.* You come, then, to enjoy the undisturbed conviction, that the creation of every original species of animals was a special act of God, and that they are, in every vital sense, contradistinguished from inorganic matter. And when you shall have thus studied nature as she is, you will find her in perfect harmony with your religious impressions; nor can she fail to exalt your religious fervor.

"Let *philosophy* interrogate nature to its fullest satiety, under the direction of its heaven-born principles; but, let it be consistent, and maintain its dignity. And should it sometimes, as it must in its wide range of nature, come in contact with miracles,—this is its limit, contented that it begins at the confines of creation; yet, still may it stretch into the regions of eternity,—past and TO COME; but now it is employed in its nobler work of sacrificing its relations to second causes, and in establishing relations with the FIRST CAUSE OF ALL." (*Comm.*, Vol. 2, pp. 132—140.)

Such, then, is a farther exhibition of the religious doctrines which it has been one object of my "Commentaries" to inculcate, and which pervade the work on Geology announced in my late "Examination."

In taking my leave, at least for the present, of the foregoing reviewers, Dr. Carpenter will indulge me with *borrowing* a sentiment from *his* Defence. Thus:—

"I trust that I have now sufficiently vindicated myself from the principal charges which the reviewer has brought against me; and that I have proved his incompetency to pronounce an opinion upon the merits of my work. More than this it is not my desire to urge. And I shall conclude with again expressing my regret at the necessity I have felt to make animadversions that so seriously affect the character of a Journal which has rendered great services to medical science, and to which the profession has been *accustomed* to look up with respect." (*Dr. Carpenter.*)

If, then, the Edinburgh Journal should be visited by the retribution which is here invoked, for the just exercise of its high prerogative, what should be the destiny of Journals which have endeavored, by an unmitigated series of misrepresentations in relation to my work, to impede the march of those principles upon which the Almighty has constituted the order of nature, and upon which He has engrafted the highest destinies of man,—and especially where the pages of one have been also shown to be encumbered with methodical plagiarism, alike offensive to reason, to truth, and morality.

Being disposed to abide the issue of the deliberate judgment of mankind, I shall incorporate the foregoing remarks with my "Commentaries," along with my "Examination."

DR. PAINE'S ANSWER

TO

CIRCULAR LETTERS

BY

DRS. CARPENTER AND FORBES.

From the "Boston Medical and Surgical Journal."

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1842.

ANSWER TO CIRCULAR LETTERS.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—Will you oblige me with a place in your Journal, for the purpose of noticing a circular letter, addressed by Wm. B. Carpenter, M.D., to Professor Dunglison of Philadelphia, dated Bristol (Eng.), Nov. 16, 1841.

That circular refers to an imputation of plagiarisms, which I considered myself warranted in bringing against Dr. Carpenter, in a pamphlet entitled "Examination of Reviews," &c. Having exposed the remarkable tissue of misrepresentations which composes the review of my "Mediical and Physiological Commentaries" (as appeared in the April No. (1841), of the British and Foreign Medical Review), and having, for very obvious reasons, drawn the offender from his obscurity, I then proceeded to inflict upon him, yet farther, what I regarded as a proper chastisement for the cowardly and wanton injury which he had attempted to perpetrate towards myself, and upon Dr. Forbes for admitting so *malicious* an article into his Journal, by exposing the plagiarism to which Dr. Carpenter's letter refers. The plagiarism was fully substantiated as it respects the Journal; and circumstantial evidence was submitted, going forcibly to show the probability that Dr. Carpenter was the author. That evidence was conclusive in my own mind, till it should be rebutted by contradictory proof; and, of course, I had no doubt whatever that the public would sustain my conclusion on examining the nature of my premises. The repeated plagiarisms occur in the elaborate reviews of John Hunter's works, and of works by Carswell, Macartney and Rasori, contained in the April and July Nos. of the British and Foreign Medical Review, 1839, and occupying sixty-one (61) pages of the Journal. These authors, too, being pretty much used up by the reviewer, I considered it but an act of justice to the brightest ornaments of our profession to remove this slur upon their fame.

It is the object of the circular letter addressed to Professor Dunglison, to disclaim the authorship of those reviews, and this statement is accompanied by a letter from Dr. Forbes to Dr. Carpenter, in which Dr. Forbes remarks that,—"I shall take no notice whatever of his [Dr. Paine's] attack, farther than relates to the charge of plagiarism. *This is true*, so far as the writer of the review on Hunter is concerned, but *false* as concerns *you*—since you did not write that review. This I am ready to state to all persons, at all times, as the truth, without any reservation or equivocation."

I have also received a curious letter from Dr. Carpenter, stating that he is not the author of the reviews of Hunter's, Carswell's, Macartney's, and Rasori's works, accompanied by copies of certificates from two gentlemen to Dr. Carpenter, expressing their belief that he is incapable of an act of plagiarism. The letter also contains a reference to an article in the Lancet of Nov. 27th, from which it appears that it is the tendency of that article to exonerate Dr. Carpenter from the imputed plagiarisms,—but which I have not seen. With the package came, also, the *certificates of character* supplied to Dr. Carpenter on the occasion of the review of his "Principles," &c., by the Edinburgh Medical and Surgical Journal.

This is all the proof with which I have been supplied in opposition to the various and forcible internal evidence of the imputed plagiarisms. This evidence I know to have been generally considered ample in this country, as it appears to have been also in London. Indeed, this fact is prominent upon the very face of the circular letter. Such proof, therefore, can only be set aside by producing some other name as that of the author in question. If my proof be insufficient, it would seem to be obvious that the name of the plagiarist should be given to the world. This is alike due to Dr. Carpenter, to men of letters, and certainly to the dignity of Dr. Forbes himself. Indeed, till then, such as are disposed to exonerate Dr. Carpenter, *must hold Dr. Forbes responsible. Indignation* at so great a fraud upon himself should prompt a *disinterested* editor to expose the offender. Why does not Dr. Carpenter call for a disclosure of the author? This is certainly a most natural, as it would be a summary, mode of disposing of the whole subject. But again, I say, *where* is the *editor's self-respect*, that he does not expose the individual who perpetrated the indignity towards him? "The conduct of the writer of that review," says Dr. Forbes, "in *palming* upon the *Editor* a portion of the writings of another for his own, *cannot be sufficiently reprobated*." Then, I reiterate, give us his name, and "let justice prevail, though the heavens fall." Present us another name; and then we shall have another phenomenon added to those extraordinary combinations of coincidences which Cotton Mather arranged under the denomination of "Unaccountables."

And yet mark the sophistry,—the effort to disguise, or palliate, the most flagrant plagiarisms to be found on record;—"if really done intentionally," says Dr. Forbes, "and with a view to deceive,—I would fain hope that the fact may admit of some other interpretation," &c.! And now let the intelligent reader consider the motives for this undisguised attempt to entrap him into the conclusion that this most degrading plagiarism may have been accidental! Let the reader then, also, interrogate himself as to the degree of credibility which should be ascribed to the man who can make such a compromise with the plainest facts, and with his own understanding of them.

And, why speak I of *self-respect* in relation to a man who professes the "truth *without* equivocation," and yet contradicts the principle in nearly every line of his letter? Does not the whole of his letter—I repeat it—bear an aspect from which truth recoils, as much as common decorum is startled at its low-born insolence? Take any passage in the solitary letter-page, and every unprejudiced mind will allow the justice of my criticism. What can be more wilfully false than the whole of the following? Thus:—

"In looking at the vast accumulation of words in Dr. Paine's pamphlet, I confess that I feel regret that the review of his book (just and accurate as I still hold it to be) was not more favorable; as it is melancholy to think that so much time and pains should have been stolen from tasks of usefulness, and expended in elaborating a work, which, of course, no human being will read, except the author himself, perhaps the writer of the inculpated article, and, alas, the Editor of the Review."

Here it is an obvious falsehood in affirming that he holds the review of my "Commentaries" to be "*just* and *accurate*," notwithstanding I have shown that the review is, throughout, a tissue of deliberate misrepresentations. Again, the opinion is not less falsely expressed, that "no human being will, of course, read the work except the author himself, perhaps the writer of the inculpated article, and, alas, the Editor of the Review." And what shall be said of Dr. Carpenter for appending this abusive letter to his circular, after the unatoned offence of misrepresenting my labors, and my character?

Again, Dr. Forbes states that my imputation of articles in his Journal to Dr. Carpenter is founded upon the editorial pronoun "*we*"; whereas, the *most important are directly claimed by Dr. Carpenter*, ***in his own works, as his productions***; and where he refers to others in his review of my "Commentaries," I have shown that it is not in the ordinary way of editorial reference, but that he sets up a *claim* to the articles in question, of which the review of Hunter's works is one. Or take the following prevarication, by which Dr. Forbes would insinuate that Dr. Carpenter is not the author of the review of my "Commentaries," instead of a manly disavowal. "Perhaps," says Dr. Forbes, "when Dr. Paine discovers that he is mistaken in the affiliation of this portion of the Review, he may feel somewhat less confident of the evidence by which he thinks he has traced the authorship of other articles in it to you. I certainly shall not GRATIFY HIS CURIOSITY on this point, by either affirming or DENYING the accuracy of his conclusions; and I DO NOT SEE ANY REASON WHY YOU SHOULD." *(My capitals.)*

And why this wily advice not to admit or deny the authorship of the review of the "Commentaries"? Was it supposed that either might possibly invalidate the statement as to the plagiarism? Nevertheless, the cunning of this advice is worthy its well-disciplined author, however it is a palpable admission of the very fact which he aims at concealing. But, I will soon add the paragraph upon which I had just commented, in connection with another from Dr. Carpenter's letter, to exhibit more fully this lame attempt to insinuate the belief that Dr. Carpenter was not the author of the gross injustice which had been done to my labors, and of which Dr. Forbes still appears insensible.

That the author of the plagiarisms, whoever he be, should broadly deny it, seems almost a matter of course. It would be absurd to suppose him restrained by principle; and it is equally important that Dr. Forbes, even with his sensitive conscience, should make it appear that Dr. Carpenter is not the author of the reviews which embrace the plagiarisms; since Dr. Carpenter having avowed himself, in the Preface to his "Principles of Physiology," the author of elaborate articles in the British and Foreign Medical Review, the proof of the plagiarism standing uncontradicted would be fatal to the *existence of that Journal.* But mark; as it respects the articles in question, Dr. Forbes affirms that those extensive reviews of four most eminent cultivators of medical science were "the first specimen he had had of this person's writing, and, with one trifling exception, the only one he had ever had"! *Credat Judæus!*

It will be recollected that the plagiarisms consist of thefts from the Rev. Dr. Channing's works, and that the imputation reaches to Dr. Carpenter's "Principles of General and Comparative Physiology." I refer to this, for the purpose of introducing the following coincidence from Dr. Carpenter's circular letter. "The ideas which I have expressed," he says, "have been so long familiar to my mind, that I cannot imagine that they involve anything peculiarly Channing-ian. If any correspondence do exist [!] it is easily accounted for by the fact, that I received my education from one, who was for many years the respected and attached friend of that illustrious man, and WHOSE *mind,* cast in the *same* mould with HIS, *impressed* MINE with those *habits* of thought, which had led to whatever SIMILARITY *may* present itself between OUR published opinions"!!—*(My capitals and Italics.)*

Now, then, this remarkable fact never would have been laid before the world, but for two obvious reasons; namely, 1st, because the parallel readings which occur in my "Examination" are *convincing;* and, therefore, we have here, under Dr. Carpenter's own signature, in his very letter of denial, a full admission that I had ample ground for the imputation of plagiarism, even had I not been prompted by the wanton attempt of this individual to falsify the hard labors of my professional life. 2nd, the foregoing remarkable fact is stated, also, because it is more or less known that Dr. Carpenter was educated by one who was "for many years the respected and attached friend of *that* illustrious man,"—but a fact which was wholly unknown *to me* till I saw it stated in Dr. Carpenter's circular!!

Again, when such fluttering occurs among the most callous critics of this or any other age, can there be a more substantial proof that my imputation of plagiarism is powerfully sustained? Do *such* critics tremble but under a well-merited lash, and this, too, when applied by one whom they affect to hold in that indifference which is the never-failing resource of defeat? Does not the whole world believe that my proof is clear, and does not the trepidation of the redoubtable critics evince their consciousness of the fact? And why does the world believe? Surely upon my *proof,* not my *dictum.* Dr. Carpenter says to me, "your charges [not *charges*] have been very generally believed among those who do not know me." And who are they that thus surrender their belief to an unknown foreigner against one of their own cherished and much-honored countrymen? Who are they that thus forego an indomitable and ever-glorious national pride, to do a mortifying homage at the shrine of truth? They are illustrious Englishmen—the most illustrious men of the age—such as *believe* only upon *proof* when character is impugned. Nor—I repeat it—was a humble republican of America at all likely to gain indulgence but upon the abstract merits of his cause. Let it then be known, that I distributed eight hundred copies of my "Examination" amongst the sçavans of Europe, and in every instance, but one or two, their names are publicly enrolled as employed in the cause of science. To those same gentlemen I shall transmit these remarks.

And yet it is possible that I may be in error, and, what is very unusual, upon such a question, the intellectual world may be in error also. Let us then inquire, which is the greater offence—an act of plagiarism by a critic, or a systematic tissue of misrepresentations, by the same critic, of one of the most laborious works that has ever come from the medical press? To prove this falsification was the *main* object of my "Examination." Why, then, so much solicitude about the plagiarisms, and none at all about the *falsehoods?* Dr. Carpenter complains, in his letter to me, of the injury which will result to him from my imputation; and had there been one word of regret in that letter at the furious assault which had been made upon my labors, and even my character, it would have paralyzed my arm forever. But, he seems utterly insensible to the injury which would have resulted to myself from his libellous attack, had I not published that "Examination" under which he is now doing a bitter penance. There is, however, no parallel in our cases. I was marked as an innocent victim by the unprincipled editor; and his ever-ready scribe rejoiced in the opportunity. The dispensations of justice were against them. The guilty have fallen; and now they come before the world with a selfishness which is truly characteristic of the trade they follow.

I trust that the public will not be led away by the wailings of wounded pride from the main object of my "Examination of REVIEWS," which was to expose the scandalous system of reviewing by a part of the London medical press, which is mostly conducted by young aspirants who endeavor to sacrifice all but their own *cliques*, or, at most, lavish their praises upon the works of others which they know to have fallen "dead-born from the press." If our *par nobile fratrum* have been caught at last, shall they be permitted to effect their escape *under a cry* that is foreign to the great object of my "Examination"? I certainly feel but *very little interest* in the affair of the *plagiarisms*, beyond the fullest disposition to be just to others, and to sustain the truth. And, while adverting to the leading medical presses of London, as well, also, to show with what consideration the imputed plagiarisms were received in Europe, I will take the liberty of quoting a paragraph from a letter which I have just received from as distinguished a philosopher as adorns the present age—premising, also, that I have not the honor of knowing him personally, or of ever having before received a communication from him. Thus:—

"DEAR SIR,—*I beg to thank you for the copy of your 'Examination' which you were so kind as to send me, and which I had the pleasure of receiving a few days ago. The exposure of Dr. Carpenter's* *plagiarism will do good. The whole system of anonymous medical reviewing in this country is disgraceful, conducted as it is almost entirely by the hands of a set of pert boys, at most but just emerged from their medical studies."*

Finally; it is unnecessary to say that there is no other attempt to escape from the proof by which I so variously identified Dr. Carpenter as the author of the review of my "Commentaries," than the following prevaricating passage with which Dr. Carpenter's circular letter commences, and which, of course, is equivalent to an admission of the authorship, however he may be disposed to screen himself behind his accommodating friend. Thus:—

"Having just received from Dr. Paine a copy of his 'Examination' of the Critique on his Medical and Physiological Commentaries, which appeared in the April number of the British and Foreign Medical Review, I find, to my great surprise, that Dr. P. has thought himself justified—not only in singling *me* out as the Author of it, and in animadverting upon what he considers to be *its* misrepresentations, as if they were *mine*, thereby attempting to make that a matter of personal discussion between us, for which the editor of the Review holds himself responsible," &c.

This is all the atonement I receive for the wrong attempted by Dr. Carpenter; whilst he holds himself up as an injured man because he was unfortunate at the game he had undertaken. As to his "surprise at being singled out as the Author," he should have considered more maturely the spirit of the following passage which occurs in my "Examination." Thus:—"That great and dignified critic, Samuel Johnson, advises authors—'to consider how they whom publication lays open to the *insults* of such as their *obscurity* secures against reprisals, may extricate themselves from unexpected encounters.' It is obvious that one of the important expedients, in cases of this nature, lies in raising the veil, and surprising the offender." That I was correct in *this* opinion, I presume that even Dr. Carpenter will most readily allow.

But, take a passage to which I have already referred for another purpose, from Dr. Forbes's letter, in which, with a view to protecting Dr. Carpenter against the charge of being the author of the review of Hunter's, Macartney's, Carswell's and Rasori's, works, he unwittingly avows that Dr. Carpenter is the author of the Review of Paine's Commentaries. Indeed, he even founds an argument upon the avowal. The authorship being admitted, Dr. Forbes then proceeds to show that Dr. Carpenter must not be held responsible for other articles because he employed the editorial *we* in his reference to others, in his review of Paine's Commentaries.

"It is singular that Dr. Paine should have been so ignorant of the ordinary mode of conducting a Review, as not to know that the reference from one article to another is no proof whatever of the identity of the authorship of the two—even when this reference is made by the writer of the latter article. But, most commonly, such references are made by the Editor, without any communication with the original writer, in the exercise of the privileges inherent in the office of the great editorial WE."

The foregoing is the shuffling to which I have before referred as misrepresenting the means by which I have connected Dr. Carpenter with various articles in the British and Foreign Medical Review. The impotent attempt which is made in the circular letters to imply that Dr. Carpenter is not the author of the review of my "Commentaries," in opposition to the various and overwhelming proof contained in my "Examination," and the simultaneous betrayal of this attempt at deception, as well as the other exposures which I have now made, divest the letters of all claim to credibility, even were not the parties arraigned for an offence which demands other proof of innocence than the mere negative of the inculpated. But, I also repeat it, these two letters not only establish the guilt of Dr. Carpenter as it respects his misrepresentation of my labors, and of my character, but go far to fix upon him the *stain of plagiarism.* In his future attempt to wipe away this stain, it will be well to express some contrition at the magnitude of the offence which was perpetrated in relation to myself.

I have always been sensible that I had but little to hope, at present, from the medical press of Europe; nor have I been mistaken in the estimate I had formed of a portion of the press in that part of the world. I have never apprehended, however, that full justice would not ultimately come, when this "*age of pamphlets*" shall have passed, and therefore I have looked with comparative indifference upon the treatment which I have received, though with a stern determination to protect myself against every act of injustice from sources entitled to consideration. That tears of editorial sympathy will now deluge the European hemisphere, cannot be doubted; but, whether the flood will be increased from this side of the Atlantic, time can only disclose. In the meanwhile, I shall prepare myself for the coming of the waters, and be ready with my Ark for a "nine days'" storm. I am, Mr. Editor, most respectfully yours,

New York, 446 *Broome street.* MARTYN PAINE.
December 24, 1841.

P. S.—I shall esteem it a favor, as well as an act of justice, if those journals which may publish Dr. Carpenter's circular letter to Professor Dunglison, or other communications from Dr. Forbes relative to the imputed plagiarisms, will give the foregoing comments an insertion.

M. PAINE.

A REPLY

TO AN ATTACK

BY

HENRY I. BOWDITCH, M.D.

UPON THE ESSAY

ON THE PRINCIPAL WRITINGS OF P. CH. A. LOUIS, M.D.

AS CONTAINED IN THE

MEDICAL AND PHYSIOLOGICAL COMMENTARIES,

BY

THE AUTHOR.

Male cuncta ministrat Impetus.—*Proverb.*

BOSTON:
REPUBLISHED FROM THE MEDICAL AND SURGICAL JOURNAL.
184 Washington Street, corner of Franklin Street.
1840.

DR. PAINE'S REPLY TO "H. I. B."

To the Editor of the Boston Medical and Surgical Journal.

Sir,—You will much oblige me by publishing the following reply to the attack upon my Essay on the Principal Writings of P. Ch. A. Louis, M.D., which appeared in your Journal on the 9th, 16th and 23d of September, purporting to have been written by "H. I. B." I have divided my communication into several parts, so that it may interfere as little as possible with the rights of other correspondents.

New York, Oct. 4th, 1840. Very respectfully yours,
The Author of the
Medical and Physiological Commentaries.

I am sensible that it is a good rule for an author to leave his work to the justice of the world, which never fails to discover its motives and its merits, whilst it holds in the same balance those of his critics. But, it has been the destiny of my book to have met with an unusual adversary before it can have had the advantage of any general perusal; whose purpose has been to impede its circulation by misrepresenting specifically my Essay on the "Principal Writings of M. Louis," and the objects of the work in a general sense. I am, therefore, induced to depart from my views of expediency under ordinary circumstances, and to protect myself against the misstatements of "H. I. B.," till the public may take the work into its own charge.

Having, then, undertaken my own defence, I shall notice every point of attack, *in extenso*, that the work may be neither imperfectly done, nor my neglect of any objection be construed into an admission of its correctness. But, more than myself, I have in view a defence of the Essay which is the special subject of assault, since it relates to some momentous questions in medical science.

It is painful to me in performing this act of justice, that I am compelled to touch upon the morals of another, or to employ any language of recrimination. It is more especially so to recur to a task which I assumed in relation to the writings of M. Louis, and which was undertaken solely in behalf of my profession, and of that general experience of the world upon which my work is founded. But, having determined not to remain silent, I must be efficient. My whole position in respect to M. Louis being susceptible of entire defence, it must not be neglected from any indisposition to renew the subject. I will endeavor, however, to avoid the expedient of my critic in substituting fraud and personalities for fact and argument. It was doubtless thought that the former would deter me from any collision with him; and here my assailant would have calculated rightly, had he not overstepped the mark as to my honor and veracity.

It may be assumed that "H. I. B." has made the best of his cause, not only for an important reason yet to be assigned, but from his declaration that "we are devoted lovers of the plan originally proposed by others, but first fully developed by Louis, viz., the Numerical Method. We hereby give in our faith," &c. Should it appear, therefore, that "H. I. B.," actuated, also, by the lowest motives, has utterly failed in all his points of attack, the work which he assails will be fairly entitled to the consideration of having placed certain dangerous innovations upon philosophical and practical medicine in such an aspect as to defy their most interested advocates. The public, therefore, will perceive that it is deeply interested in this matter; and in proportion to the intrinsic importance of a subject, is ever its disposition to sustain the advocate of truth,—and especially so, where it is apparent that he has had at heart the well being, alone, of his fellow man. But, I ask for no sympathies. My defence looks only to simple justice, and at questions in which the medical public is as much interested as myself. It is therefore especially upon this principle that I expect a patient hearing, and an impartial verdict. If that verdict be my exoneration, it will then appear that I have been hitherto employed in endeavoring to advocate the cause of truth, the violated dignity of the profession, and the desecrated worth of its illustrious founders and of those devoted men who have carried the science of medicine to its present high advancement. In the Essay in question I have endeavored to turn aside the scorpion lash, and here, as elsewhere, to rescue medicine from its imputed "infancy," and to place it where it belongs, in virtue of its wide range of observation and surmounted difficulties, at the head of all human pursuits. This is its rank,—not because of its abstract importance to the necessities and the happiness of mankind, but because no other science was so early cultivated with zeal; no other whose foundation was originally laid in the Baconian philosophy; no other to which that philosophy has been almost uninterruptedly applied for more than two thousand years; no other upon which the efforts of genius have been so largely and powerfully concentrated; no other which swallows up all other science; no other which has left so little for the labors of posterity. I speak of its proper attributes,—not as they are illustrated by ignorance or speculation, or as they are misrepresented by ambition or honest credulity.

I shall have little to say of the style and temper of my reviewer. They carry their own elements of interpretation; and, whilst they seem to denote an unexpected calamity, they have probably rendered sufficiently suspicious the pretences which they were designed to enforce. I come, therefore, at once, to the subject matter of *complaint*. In the first place, however, I would say that, "H. I. B.," the author of the strictures under examination, having impugned my veracity, should have had the magnanimity to have written without disguise. The initial signature, although sufficiently indicative to many of the authorship, is concealment to most, and may be soon unknown. It serves, however, as a pretext for open avowal should the strictures remain unnoticed, and as a loop-hole of escape should a merited exposure be made. I shall therefore place the authorship upon a ground that shall be intelligible to all, and that no innocent man may be unjustly suspected. I assume, then, that "H. I. B." is Doctor Henry I. Bowditch, the Translator of Louis on Typhoid Fever, who also "revised and altered" Cowan's translation of Louis on Phthisis, and who is or was the copyright proprietor of the American editions of those works. The ground of my conclusion as to the identity of the writer is the initial signature to the review, its appropriation by Dr. Bowditch in his notes to the foregoing works, its remaining uncontradicted, its undoubted import by others, and the internal proofs which abound in the article.

Dr. B. professedly undertakes the defence of M. Louis. After saying that he has "no fears for the foundation of Louis's main ideas,"* he continues,—"Still we feel unwilling to allow any writer, under the pretence of examining the philosophical views of M. Louis, to traduce his character. If all that is said by Dr. Paine of the former physician of La Pitié be true, M. Louis deserves to be treated not merely as one wholly unworthy of confidence in medicine, but as an individual of a base private character."—(P. 73.) And farther on he repeats, "Dr. Paine attempts to prove Louis a liar (we are aware of the meaning of the word), by quoting a translation."—(P. 80.) Here, then, Dr. B. has unnecessarily magnified the inductions which I had drawn, and perverted my Essay to the injury of his friend, since I certainly neither expressed nor entertained such an opinion; and the best evidence which I can offer of my reliance upon his veracity is to be found in the unhesitating manner in which I have received all his statements. This confidence in his veracity appears, also, repeatedly in other Essays, where I have ever quoted his facts and opinions as substantial authority. (See Vol. 1, pp. 314, 408, 533; Vol. 2, pp. 320, 626, 629, 632, &c.) And when employed in repelling his imputation of duplicity and falsehood to venerated members of the profession, I have not retorted the allegation, except in a suppositious manner to show the unhappy results of discrediting medical testimony. (See Vol. 1, pp. 295, 331—332; Vol. 2, p. 680—681.) We have constant examples before us, and around us, where ambition so far saps the moral principle as to render it careless of public rights; but where, also, it is restrained in respect to those private virtues which form the important cement of society. Shall we, for instance, pronounce the politician a "liar," and of "a base private character," because he disregards the public weal to carry the objects of party, or even to advance the aim of selfish ambition? It is exactly in the foregoing respect that I have considered M. Louis,—publicly wrong, but probably amiable in private life. This is one of the paradoxes of human nature, which could not be inferred *à priori*, but which is revealed through daily experience. However unhappy this system of morals, the broad limits exist as I have represented them; though it be certainly true that the barrier is easily passed, and the distinction obliterated.

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* The introductory part of Dr. B.'s attack is a common reëcho of what appears in Cowan's Preface to the work on Phthisis, in relation to Broussais's *Examen*. "Whilst thus deprecating," says Cowan, "the tone of prejudiced and illiberal feeling which pervades the strictures of M. B——, *we are not anxious about their effects* on the volume before us," &c. The notice, however, is respectful, notwithstanding Broussais "has alternately impeached the accuracy and *veracity* of M. Louis."—(P. 59.)

What I have said of M. Louis's ambition as a public man, of his diplomatic expedients, of the rights he has invaded, of the characters he has assailed, of the degradation to which he would reduce the profession, is predicated of premises which are before the world, in the language of my author. That world, and not an interested partizan, must be my judge. And now, to do the fullest justice to all, I may say that one of my author's judicious friends and defenders, in a late notice of my Essay, does not intimate that I had questioned his veracity, and allows my "general fairness and candor."*

But, what is most extraordinary, is the affirmation, that, "Dr. Paine attempts to prove Louis a liar *by quoting a translation;* whereas, if he had taken the trouble to look at the original, he would have found that he by no means could prove what he wished."—(P. 80.) Now Dr. Bowditch being the author of that translation must certainly take the imputed blame to himself. Least of all should he put forth his own version as the ground of his author's error, and by thus repudiating it, endeavor to show that through his own inaccuracy he had led a confiding man to do an act of the highest injustice to another, and then turn upon him as a calumniator. It will be seen, also, that this matter relates to a question in which M. Louis has taken no part, and is one with which his veracity has not the slightest connection. That nothing, therefore, is found by Dr. B. to justify his imputation is manifest from its guarded manner, the nature of the premises, and the subject matter which will be soon set forth.

I had certainly some suspicion that Dr. B. would complain of any version I might have attempted of the writings of M. Louis,—especially those on Phthisis and the Typhoid Fever. I therefore took the precaution of adopting his own translations; and this more especially as we are assured by Dr. Bowditch in his Preface to the latter, that he believed he had "translated *truly the meaning* of the author *in every respect*," whilst that on Phthisis had been "revised and altered" by the same hand, and duly registered in the archives of the nation. Having thus determined upon the *safest* course, and stated the fact at page 686, and no objection having been raised against the translation in Europe or America, till now made by the translator himself, it is clearly absurd to suppose that it was incumbent on me to have compared it with the original work, since this would have involved the necessity (on account of the multiplicity of my extracts), of comparing the whole of the four volumes on the typhoid affection. My objector says, "If he had taken the trouble," &c. The task is more difficult than the requisition to perform it, since the principle exacts not one, but an universal comparison. And suppose that others, as Müller, for instance, whom I have quoted largely and critically, should start the same objection; will it be said that it was my duty to have compared the whole of Baly's translation of 850 compact pages with the original German; and especially when I stated in my work that I should adopt acknowledged translations of foreign authors in preference to any version of my own, that no one might complain of misconstruction. Dr. B., however, having condemned his own translation, renders it proper to inquire how far he may escape under this subterfuge, and especially whether he has made out his principal, but more concealed object.

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* New York Journal of Medicine and Surgery, No. 6, Oct. 1840, p. 423.

The reader, by referring to the connection of the foregoing quotation with other remarks by Dr. B., will observe that the imputed attempt to convict M. Louis of "lying" is predicated of the proof by which I thought it expedient to identify M. Louis with the superintendence of the typhoid cases. Since, however, M. Louis no where intimates that he had not the entire charge of these cases (the only ones which I have assigned him), but, on the contrary, everywhere implies that such was the fact, the charge upon which I am arraigned is necessarily a fabrication, and as criminally injurious to M. Louis as unjust to myself. What, then, was the object of Dr. B. in this experiment? Could there have been any other than that of forcibly implying that M. Louis had denied his care of the typhoid patients and that I had endeavored to prove it otherwise? But, a greater consequence is involved, viz., that of impressing the reader with the certainty that M. Louis was not the prescribing man. The question, then, had not the most remote connection with my author's veracity. So unsuspicious, indeed, was I of "attempting to prove M. Louis a liar," I had supposed that I was merely employed in receiving his own testimony that he was responsible for the treatment of the 138 typhoid cases, upon which he erects his generalizations in relation to the typhoid fever. Dr. B. professes not to understand my motives even for this. I will tell him, what the reader, however, who may have seen my Essay, sufficiently comprehends. About the time my work was going to press, in a conversation with one of M. Louis's able disciples, I told him that I considered Louis not only vulnerable as a pathologist, but in his treatment and therapeutical conclusions. The defence was immediately set up, that Chomel was the hospital physician, and consequently alone responsible. I had little doubt, therefore, that Dr. B. would be very likely to resort to the same pretext. Indeed, I have stated this very fact in my Essay (p. 800). "*Hinc illæ lachrymæ.*" In every aspect of the case, however, it should be the pride of his followers to admit, what Louis constantly implies, that he had the active superintendence of the 138 cases, since his "rigorous conclusions," anatomical, pathological, and therapeutical, would be, *prima facie*, seriously impaired without this evidence of their practical foundation.

Still, it is manifest that Dr. B., for divers reasons, is strongly disposed to avail himself of the pretext; by which the reader will perceive that I had not been greatly mistaken in my conclusion, nor unwise in my precautions. Thus, in connection with the quotation which leaves me impeaching the veracity of M. Louis, Dr. B., to give it plausibility, affirms that "there is no such expression in the work on typhoid fever, as 'I prescribed,' meaning thereby to state definitely that Louis prescribed." It is here that Dr. B. disavows his own translation of an instance which I had cited to the foregoing effect, and one of a very comprehensive nature,—namely, "*we abstained* from bloodletting in the others, either on account of," &c.,—the original being, in this instance, "*On abstint*," &c. Again, a little farther on (p. 448), M. Louis says, or his translator for him,

"We allow generally that a moderate delirium does not require a special and very energetic treatment, and according to the most common practice, *we confined ourselves*, in order to counteract it, to the application of blisters and sinapisms to the lower extremities, in the patients *whose histories* I have compiled."

I have also taken this example on account of its comprehensive import, and have confined myself to Dr. B.'s translation, not only because I originally proceeded upon that principle as the one which I supposed would be most acceptable, but to show that, if the version be not exactly literal, it was rendered otherwise from the conviction of Dr. B. that Louis was the "acting man" in relation to "the patients whose histories he had compiled,"—whilst we have the doctor's affirmation that, in his belief, he "had translated *truly the meaning* of the author *in every respect*."—(*Preface.*)

I am now no farther interested in this question than in carrying out my intention of simply performing an act of justice to Chomel. If this gentleman chooses to take the credit of the 138 typhoid patients,—be it so. The principle, as to the only object of my analysis of the treatment, will be in no respect affected, unless it be to diminish still more the value of my author's therapeutical generalizations. But, till then, I must insist upon the evidence for my conclusion, that the treatment was not only the unqualified basis for doctrines of the highest moment, but was projected by M. Louis himself. This, indeed, is everywhere so apparent from the manner in which M. Louis identifies himself with the history of every case from the time of its admission till its exit, that the fact cannot be doubted till contradicted; and this will never be done by M. Louis, or by any considerate friend, not only for the reason already stated, but as it would imply an attempt to have misled the profession. Take, again, the following examples of the manner in which the translator "*truly*" understood "*the meaning*" of his author. Thus,—"*Our* patient had kept his bed," &c.—(Vol. 1, p. 4.) "*Our* patient was placed," &c.—(P. 6.) "Although *our* patient was placed," &c.—(P. 68.) The work abounds with expressions which appear to denote that Chomel extended his kindness so far to M. Louis as to suffer him to institute his experiments upon these 138 patients. The reader may find the proof on almost every page. I referred specifically to a multitude of instances. Vol. 1st of the translation may be consulted at pages 21, 45, 64, 82, 85, 87, 95, 121, 132, 148, 159, 237, 244, 263, 343; and vol. 2d, at pp. 7, 14, 27, 29, 30, 39, 45, 60, 74, 77, 128, 129, 136, 152, 162, 171, 180, 187, 193, 197, 198, 200, 202, 205, 206, 207, 208, 234, 261, 274, 290, 291, 307, 342, 343, 382, 384, 391, 438, 439, &c. The foregoing references relate generally to instances which show an intimate connection of the observer with the prescriptions which are announced. I will cite one for the purpose of showing also the *means* of information upon which M. Louis has founded his hypothesis as to the *primary* seat of the typhoid affection in a lesion of the glands of Peyer. After detailing the seven days' symptoms with which the patient had been affected before entering the hospital, M. Louis continues,—"The *patient* assured *me* that he had had a little *delirium*." The same day we read "slight stupor," "*memory seemed* to return to the patient as *I questioned* him," &c. Then follows immediately the prescription. (Vol. 1, p. 342.) *(My Italics.)* No one can take up the work on Typhoid Fever without feeling irresistibly that M. Louis had the active superintendence of the cases which he describes. I cannot find Chomel alluded to beyond the "Advertisement," except in three instances, and then only very incidentally. Nevertheless, I was indifferent to the fact, except so far as I felt disposed to silence prevarication, as will appear from my declaration that,—"this would be *unimportant*, since our author makes the cases entirely his own, erects generalizations upon them, and defends the whole treatment." —(P. 685.) This construction, however, Dr. B. studiously conceals. But his objects, and mine, are now before the reader. My comments upon the treatment, from which I was disposed to separate Chomel, never would have been made in relation to the practice of any man who had not treated the profession as discourteously as I have shown of M. Louis; and had this distinguished individual been less unceremonious, my Essay would either have had no existence, or would have appeared in a different aspect. What I have set forth of his hostility to the profession is urged as a merit by Dr. B. when he says, "Louis, *disgusted* with the uncertainty prevailing *in all* branches of medicine," &c.— (P. 79.) *(My Italics.)* This shows that my strictures were wanted; and if the profession do not find them acceptable, I can only regret that I had attempted so ungracious a service.

The reader will have seen that it was the apparent object of Dr. B. to pervert the foregoing subject so as to extort the charge that I had attempted to "prove M. Louis a liar." So far it was designed to impute to me what I was not attempting; but, a greater object doubtless was to establish the impression more fully, that M. Louis is not responsible for the treatment of the typhoid cases. The secret of this will be found in my comments, and in the death of more than one third of the patients. The solicitude of Dr. B. upon this question may be seen in his hunting the work through to ascertain that there is no such expression in the original as "I prescribed." The whole state of Dr. B.'s mental operations will be farther disclosed by my next exploration.

In connection with the foregoing subject, it is said by Dr. B. that "Chomel, notwithstanding Dr. Paine takes it upon himself to declare to the contrary, is *always the chief physician*."—(P. 79.) This is not only a positive averment, to convict me of untruth, but is intended to carry out the impression, as implied in the statement already examined, that I had attributed the superintendence of the hospital patients, in a general sense, to M. Louis. Should it, however, turn out in all respects exactly the reverse, Dr. B. must occupy a worse position than he had provided for myself. In the first place, then, I have carefully restricted M. Louis's agency to the 138 typhoid patients; and, secondly, I have stated the very declaration of M. Louis, upon which Dr. B. brings his charge that "Dr. Paine takes it upon himself to declare to the contrary." Thus,—"It should be observed, *in the first place*, that in the 'Advertisement' to the work, Chomel is spoken of as having *the supervision of the hospital*."—(P. 685.) Now, what says the 'Advertisement'? "At that time under the superintendence of M. Chomel;" or, as the translator has it in his new version (and as will appear more fully hereafter), "under the *supervision of* Mons. Chomel." This is every word that appears upon the subject, and, as will be seen by the entire extract in my second number, is noticed by M. Louis in a transient manner. This well-known fact I have no where questioned, and it was stated by me for the very purpose of contradistinguishing the 138 typhoid patients from the general mass, which I had never doubted were under Chomel's exclusive superintendence. And here it is worthy of remark, that Dr. B., in the very midst of this serpentine course, not only allows more than I had contended for, or believed to be true, but contradicts the whole of his statement, in saying that M. Louis "gave up his business, and entered as *clinical aid* to his friend Chomel." Again, the same thing is implied in the following sentence,—"but it remains for Dr. Paine to find out that Chomel ought to be ashamed of his *co-laborer*," &c.—(Pp. 76, 79.)

Finally, the work on Typhoid Fever was designed to unsettle the most important principles in physiology, pathology, and therapeutics; and, not being otherwise stated, it is inferable, *à priori*, that this vast enterprise had the advantage of the fundamental requisite,—that is to say, the author's personal superintendence of the 138 cases of the typhoid disease. I need not inform my reader, that it is not unusual in French hospitals for the chief physician to delegate a certain class of patients to other physicians for the work of experiment. This fact, and its consequences, I had occasion to illustrate, by example, in my "Commentaries."—*(See Vol.* 1, *p.* 305.) I may say, however, that, as in my Essay on the writings of M. Louis, I had in view no partial objects, but principles co-extensive with the importance of the healing art.

I have dwelt long, perhaps tediously, upon the foregoing implications; but they had been sedulously involved, and put forth with a commanding confidence. A full analysis has been therefore indispensable, not only to develope the motives and deportment of Dr. B., but to place my relations to M. Louis, as a commentator, in their proper aspect. Nor can I yet conclude, without admonishing the reader that something yet remains to illustrate the *quo animo* of my critic in respect to the present subject, and by which it will appear that, through a fraudulent translation and surreptitious quotations, designed to pervert the truth in other respects, he simultaneously enforces the belief that M. Louis was not only intimately concerned with all the cases of every disease to which he refers in his work on the typhoid affection, but, in so doing, imputes to that work a foundation which has no apparent existence. The statements and allegations are ingeniously conceived, and artfully involved. They require, therefore, a more or less elaborate examination. There is no one which is not essentially destitute of truth, and they are generally distinguished for a combined purpose of impugning my honesty or veracity, of misrepresenting my work, and of misleading the profession as to M. Louis's labors and doctrines. It is true, that in certain instances I am allowed the alternative of having read my author carelessly or not at all. But this is no part of the character of my work; and, whilst I am not ambitious of the indulgence, its sincerity is contradicted by the ample evidence in my Essay that I had not only read my author repeatedly, but with careful attention. The critical press, also, which has been thus stealthily invaded, requires redress; and I cannot but think, therefore, where so many wrongs are committed, the public must feel an interest in their proper exposure. I lament the necessity of employing any language that may be offensive to a chastened taste; but, vulgarity may not always be rebuked by forbearance, and there are transgressions which are better described by their right names, than by any circumlocution.

No. 2.

I SHALL now take up the examination of a subject to which I referred at the close of my remarks in the preceding number. The charge which is conveyed, and the manner of its execution, being designed as the most fatal stab at my integrity and my work, the reader must bear with me whilst I place upon record one of the most remarkable instances of critical chicanery that has probably ever fallen under his observation. I could expose the fraud, and thus cover the whole of Dr. B.'s article with confusion, in a few words. " When a man," says Dr. B., " proves false in one position, we suspect him in regard to others " (p. 107); or, as Quintilian has it, " *Maledicus a malefico non distat nisi occasione.*" But it is due to the profession, which is deeply interested in subjects of such importance as are involved in the writings of M. Louis, that I should show, unequivocally, that I have treated them with the most scrupulous fairness. It is due to men of science, and of laborious intellectual industry, that I should make a full representation of the fraud which is here practised, that the pen of interested or unprincipled critics may be less frequently envenomed, and that I should erect a gibbet at which envy itself may stand appalled. It is due to M. Louis, and to the readers of his works, that I should fully expose the manner in which the translator perverts them to interested purposes. It is due to truth, to my honor, and to my work, that I should analyze throughout an artful contrivance by which Dr. Bowditch attempts a plot of such comprehensive design. The specifications which I shall establish are,—

1st. A perversion of a pretended and fundamental extract from my Essay, in disconnecting a part of a sentence from its indispensable relation to words immediately preceding.

2nd. An erasure from the body of the extract of the two most important words, by which the whole meaning is altered.

3rd. That, by this act it is endeavored to be shown, in part, that I have fundamentally misrepresented M. Louis, and, therefore, that my Essay, if not my whole work, is radically defective.

4th. That this intended inference from a surreptitious quotation is directly enforced by an accompanying declaration as set forth in my third specification, and that this charge is also of a general nature.

5th. That a passage of specific import, and relating *exclusively* to the ostensible ground of the supposed anatomical lesion of the glands of Peyer, is put forth in a mutilated form, to show that I had given "an unfair impression of his (M. Louis's) labors;" when, in its appropriate place, I had allowed more than is required by M. Louis, or by Dr. B. himself.

6th. That the offence which Dr. B. was thus imputing to me, was perpetrated in the very act and in an unexampled manner by the projector.

7th. That the charge is sustained by a false translation of the original French; and that, in thus falsifying the author, an impression is attempted that I had committed the crime.

8th. That the translation, in its most essential attribute, is wholly different from the original translation by the same hand, which had declared in its Preface the belief that it had "translated *truly the meaning* of the author in *every* respect."

9th. That the perverted translation ascribes an importance to the supposed characteristic anatomical lesion of the typhoid affection, which is not even insinuated by M. Louis.

10th. That the perversion of the translation, and the induction from it by Dr. B., appropriate a foundation, in a general sense, to the work on Typhoid Fever, which M. Louis never designed in the sense which is thus conveyed.

11th. That Dr. B. employs a mutilated fundamental quotation, and a false translation, as a principal means of convicting me of falsehood, when the true quotation and the true translation show, respectively, that I have stated the truth.

12th. That, contrary to his usual practice in the review, Dr. B. suppresses the original French in the instance under consideration, and a reference to the page in my work.

13th. That, as a critical writer, Dr. B. has adulterated a most important element of the press, thrown suspicion over its honest guardians, and by thus blunting the edge of criticism he has opened wider the door for licentiousness in writing.

Dr. B. is speaking specifically of the supposed characteristic *anatomical lesion* of the typhoid affection.—(P. 82—83.) He goes on thus: "Dr. Paine may think he has gained his end, and that Chomel and we both allow that Louis generalized too quickly—and that in stating the anatomical characteristic of the typhoid fever to be a lesion of Peyer's patches, we declare that the symptoms are dependent upon this change of these patches. Now let us examine Louis's works and see what he says upon the subject; and first, we must say that in stating the foundation of Louis's assertions, Dr. Paine, as usual, gives an unfair impression of his labors, and leads the reader to believe that all the cases of any disease that Louis examined in order to arrive at definite conclusions in reference to the characteristic lesion of the typhoid disease, were '50 cases of acute disease having certain other analogies, and 83 other cases where these analogies are said to have been more or less wanting.'" The latter clause of the sentence from "50" embraces my words, which stand in their proper relations, and bear the usual marks of a quotation.

Now I will show by the statement as it exists in my work, that I represented the whole truth, and nothing but the truth. The pretended quotation is at my page 686, and refers *exclusively* to the number of cases which are offered by M. Louis as the absolute foundation of his generalization of the *anatomical* lesion of the glands of Peyer. Thus: "The first thing, we say, which excited our surprise, is the broad affirmation that a lesion of the glands of Peyer may be taken as the anatomical characteristic of typhoid fever, *because it was present in* 50 cases of acute disease having certain other analogies, and *was absent* in 83 other cases where those analogies are said to have been more or less wanting."—In the same paragraph, and connected with the same subject, I use the words, "a comparatively *unimportant* lesion of structure;" that is, of Peyer's glands. The bearing of the word "*unimportant*" will be seen hereafter.

The foregoing 50 and 83 cases (133) make the whole number of dissections upon which M. Louis professes, in his work, to have founded his conclusion as to the glands of Peyer. The reader must not be drawn off by any considerations relating to the experience of M. Louis beyond what is the ostensible foundation of his work. With this alone I was concerned; though it will be ultimately seen that I allowed any latitude of experience that might extend beyond the work itself. I should add, however, that I have objected to the employment of cases which do not appear to have received that attention from M. Louis which was bestowed upon the 138 cases of the typhoid affection, or which are not analyzed according to his requisitions of other writers,—exempting, also, from objection the 83 anatomical inspections in other diseases. It is therefore a primary object with Dr. B. to make it appear that all the cases mentioned in M. Louis's "Advertisement" actually came under his critical observation.

But, to the point. What does M. Louis say of the basis of his conclusion as to the anatomical lesion of Peyer's glands? I will first state it as expressed in the conclusion of a paragraph, which I will render memorable by soon quoting in full. Thus:—"so that I have analyzed *the diseased changes* of the viscera of 133 subjects, and the *symptoms* of nearly 900."

Now as the *anatomical* lesion could only be ascertained by dissection, the reader will see that I not only stated the entire number of cases which were the ostensible basis of the conclusion, but distinguished the *classes* of cases which make up the 133 anatomical inspections. This, as will be seen, was repeated in other places. I did it for the purpose of precision, and that the whole merits of the subject should be distinctly before my reader. But, as will be soon seen, Dr. B. endeavors to instil the belief that the whole 900 cases should be taken into the account as it respects *the anatomical* lesion of the glands of Peyer, and then charges me, directly, with having in my statement suppressed a part of the facts, or, rather, with having misrepresented them.

Let us now go back to the preceding quotations for the purpose of making out my specifications as to the extract from my Essay. The fraud consists in not taking along the words "*because it was present in*," by which the 50 cases are shown to have been instances in which the anatomical lesion was found, and in expunging from the very body of the quotation, the important words "*was absent in*." But, shall charity come to the aid of my critic, and shall he be indulged with the allowance that the quotation was carelessly made? The selected words are all arranged as they stand in my Essay, and are duly marked, whilst the deliberate mutilation of the extract will be fully shown by its artificial relation to the perverted extract from M. Louis.

The reader will therefore see, that only 133 cases can be brought to the anatomical lesion, and that I might thus silence my adversary at once by throwing his mutilated quotation back upon him. But, I have yet a long account to settle with him, and have chosen not only to expose the whole of this affair, but, in so doing, to show to the world with what scrupulous fairness I have treated M. Louis.

After the surreptitious quotation from my work, Dr. B. goes on thus: "After speaking of the state of doubt in which physicians were in reference to fever—some calling it a gastro-enteritis, others a putrid adynamic, ataxic and typhoid fever, Louis continues thus."

Having now arrived at the remaining ground of my specifications, I shall present the quotation as rendered by Dr. B. in his original translation (having *then* "translated truly *the meaning* of the author *in every respect*"), and the perverted version, in parallel columns; and then subjoin the original French which Dr. B. suppresses. The original punctuation is observed throughout.

Rejected Translation.	"*Revised and altered*" *Translation.*
"I collected during this space of time, in addition to some incomplete facts, 138 observations of typhoid fever, 50 of which were relative to individuals who had died of it. I analyzed all, and in order to determine among the numerous lesions found in the patients who died of those that are peculiar to typhus, I compared them with the alterations found as consequences of other acute	"During this period I obtained, with the exception of some imperfectly recorded facts, 138 observations of the typhoid fever, 50 of which related to individuals that died. I analyzed both, and in order to know, among the numerous lesions found in those who died, those that were peculiar to the typhoid affection, I compared them with the alterations observed in consequence

diseases, in 83 subjects whose histories *I learned.* *I did the same with the symptoms* observed in those affected with typhus fever, or with any other acute affection, which terminated in restoration to health or in death, so that I have analyzed the diseased changes of the viscera of 133 subjects, and the symptoms of nearly 900."	of other acute diseases, in 83 subjects, whose cases *I carefully recorded.* *I did the same when examining* the symptoms in patients affected with the typhoid disease or any other acute affection terminating fatally, or by return of health. So that in fact I have analyzed the alteration in the viscera of 133 subjects who died, and the symptoms of nearly 900."

The suppressed Original.

"J'ai rassemblé, dans cet espace de temps, à part quelques faits incomplets, cent trente-huit observations de fièvre typhoïde, dont cinquante relatives à des individus qui ont succombé. J'ai analysé les unes et les autres, et afin de connaître, parmi les nombreuses lésions de ceux qui ont péri, celles qui sont propres à l'affection typhoïde, je les ai comparées aux altérations observées à la suite d'autres maladies aiguës, chez quatre-vingt-trois sujets dont j'ai aussi *recueilli* l'histoire. *J'ai fait le même travail pour les symptômes*, chez les malades atteints de fièvre typhoïde ou de toute autre affection aiguë, terminée par le retour à la santé ou par la mort; en sorte, que j'ai analysé les altérations des viscères de cent-trente-trois sujets, et les symptômes de prés de neuf cents." —(P. ix.) *The Italics are mine.*

The reader will perceive that the old translation is the true one, whilst the new translation is false, and adapted to the mutilated quotation from my work. Doubtless, Dr. B., by altering the style of the whole, and by suppressing the French, expected to escape his own snare. It is also wrought with so good a device as to require some management to secure effectually the proper game. I shall, therefore, in the first place, point out one of the most important violations of the original, and then show its application. This violation consists in substituting "*I carefully recorded*" for "I learned,"—the original being "*j'ai recueilli.*" As this will prove to be an important word, it may be well to satisfy all that my critic's old translation is right, by giving the various import of the word from Chambaud's large dictionary. Thus:—"*Recueillir. To collect, to pick up, to compile, to gather.*" It will soon be seen, also, that Dr. B. understands its true meaning, as he had just previously rendered it, in the old translation, "*I collected.*"

Now the reader must turn back to the mutilated and the true extract from my work, by which I was pointing out M. Louis's data for his generalization as to the supposed anatomical characteristic of the typhoid fever,—that is to say, "50" and "83" dissections. This statement of mine, therefore, has nothing to do with any other foundation that may constitute the general basis of M. Louis's work. That, as I have said, is stated in its appropriate place. But, Dr. B. takes this extract, mutilates it, and places it at the head of the foregoing mutilated extract from M. Louis, to sustain his accompanying charge that, "Dr. Paine, as usual, gives an unfair impression of his (Louis's) labors, and leads the reader to believe that all the cases of any disease that Louis examined," &c.

The false translation of *j'ai recueilli*, or "*I carefully examined*," is designed for a triple purpose; the first of which is, that it should bear upon the mutilated extract from my work, and the imputation of Dr. B. that I had given by it a false account as to the number of cases employed by M. Louis in arriving at his generalization as to the anatomical lesion of the glands of Peyer; secondly, that I had also given by it "an unfair impression of his (Louis's) labors," in a general sense; and, thirdly, that the whole 900 cases are as much entitled to be regarded as a genuine foundation of the work on typhoid fever, as the 138 cases of that disease which M. Louis did probably *carefully record*, and whose treatment, there can be no doubt from his work, he conducted himself. It is this third bearing at which Dr. B. especially aims. Having conveyed the false impression, that I had suppressed all allusion to any other than "50 cases of acute disease having certain other analogies, and 85 other cases where those analogies are said to have been more or less wanting," he proceeds to alter the extract from M. Louis to show that the whole 900 cases were of equal value, and thus to enforce the more strongly the injustice he imputes to me. The work on the typhoid fever shows, on every page, the close attention which M. Louis bestowed upon the 138 cases of that disease; whilst we are only told, in the foregoing extract, and no where more specifically, that he "learned" or "collected" the residue of the 900 cases;—from which it is clear that he collected them from the *records of the hospital*. In any event, we are no where given to understand that he "*carefully recorded*" them himself, whilst the intent of the translator will become more glaring by his fabrication of words, and another false translation of *j'ai recueilli*, which will soon be stated. Nevertheless, I made no objection to the 83 dissections in which the lesion of the glands of Peyer was wanting, although we are only told by M. Louis that he "collected" their histories. I make this statement to show the bearing of the new translation, "in 83 subjects, whose histories *I carefully recorded*." This refers directly back to the 138 cases of typhoid fever which were the peculiar subjects of M. Louis's attention (according to the work), and, by the new words, it is implied that the 83 cases were not less the subjects of his care. The sentence then runs into the next following, so as to make it appear that M. Louis as *carefully* observed the symptoms of the whole 900 cases. Thus:—"in 83 subjects, whose cases *I carefully recorded. I did the same when examining the symptoms* in patients affected with the typhoid disease *or any other* acute affection terminating fatally, or by return of health." But, that my critic might make this conclusion the more certain, he adds another bait, and actually fabricates the words "*when examining*,"—whereby he makes M. Louis not only to have "*carefully recorded*" all the cases, but to have actually "*examined the symptoms*" of the 900 cases. Nor is this the worst of it; for, by interpolating the words "when examining," Dr. B. perfects the design at which he was aiming, namely, that of making out a fact, that M. Louis not only "*carefully recorded*" all the cases, but that he made the record "*when examining* the symptoms" of the 900 cases. Thus:— "in 83 subjects, whose cases *I carefully recorded. I did the same when examining the symptoms*," &c. The reader, by reverting to the extract, will see, also, that the words "when examining," in their relation to the preceding sentence, are even of more comprehensive import; as they represent M. Louis actually *analyzing*, *comparing*, &c., "*when* examining" the symptoms of the 900 cases.

The reader will now see the connection of the whole, by referring to the old and new translations, and the mutilated extract from my work. I may say, however, that having thus *fixed* the whole matter, the Doctor proceeds to apply it after the following manner; viz. "One would think that the *accurate examination* of about 1000 cases, and the autopsies of 1–10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not." The word "*unimportant*" is in Italics, and refers to my use of it in relation to the anatomical lesion of Peyer's glands, in the paragraph from which the mutilated extract is taken. The reader will now see, therefore, the impression which is intended to be conveyed, that, in stating the number of 133 cases as the basis of induction as to the *anatomical* lesion of Peyer's glands, I had suppressed a part of my author's data. But, a greater offence consists in representing that extract, mutilated or unmutilated, as announcing the foundation of the whole of my author's work, and in then proceeding to confirm that imputation by a false translation of a long paragraph which had no relation to my specific subject.

The reader will not be surprised to learn that I have not yet presented the whole of the fraudulent extract. The foregoing is the last part of it, and the following is the first part.

Rejected Translation.	"*Revised and altered*" *Translation.*
"In order to be able to make up my opinion upon a question concerning which one could decide very little by means of simple discussion, *I collected* between the years 1822 and 1827, the histories of all the patients suffering from acute diseases, which were admitted into the hospital of La Charité, in the wards St. John and St. Joseph, at that time under the superintendence of M. Chomel."	"In order to make up my mind upon a question which simple discussion would not tend to elucidate, *I examined and recorded*, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité *in the apartments under the supervision* of Mons. Chomel."

The suppressed Original.

"Afin de savoir à quoi m'en tenir sur une question que ne pouvaient pas beaucoup éclairer de simples discussions, *j'ai recueilli*, de 1822 à 1827, l'histoire de tous les sujets atteints de maladies aiguës admis à l'hôpital de la Charité, dans les salles Saint-Jean et Saint-Joseph, alors confiées à M. Chomel."

The reader will perceive that the old translation is the true one, whilst the new is false; the words "*I examined and recorded*" being substituted for "*I collected.*" This was designed, also, to make it appear that the 900 cases should be considered as having been "examined and recorded" by M. Louis, just as the 138 cases of typhoid fever had been, and that the whole, therefore, should be allowed to form equally the basis of his work. To carry this effect more fully, my critic has placed the words "*all the patients*" in Italics in the new translation,—the same being in the Roman letter both in the old translation and in the original work. The other Italics are mine. By this process, also, he strengthens the charge as to my having falsely represented my author as to the foundation of the anatomical doctrine, specifically, and in a general sense as to the common basis of the work.

It is also worthy of remark, that the translation of the words *j'ai recueilli* "I examined and recorded in one instance, and "I carefully recorded" in the other, and the connections in which they stand, show forcibly the *quo animo* of the writer; whilst, if any charity could be extended to the translation of *j'ai recueilli* "*I carefully* recorded," it is dissipated by the translation of the same words "*I examined and recorded*," and the farther fabrication of the words "*when examining*." But, the only meaning which *j'ai recueilli* can possibly bear is that as artlessly rendered by Dr. B. in his original version; and that such is truly his opinion is shown by its translation, in one instance, "I collected," and in the other, "I learned,"—both occurring in one paragraph, and relating to the same subject.

This affair being destined to occupy a conspicuous place amongst literary curiosities, I will indicate one aspect more in which my critic has *fixed* the matter for my discomfiture and for the benefit of "copyright." This will appear by taking together the whole paragraph which immediately follows the quotation which I have been employed in *unfixing*. The reader will then have the whole before him, from the beginning of the mutilated extract from my work, to the end of the subject in its direct aspect. The following words which I have placed in Italics are intended by Dr. B. to confirm M. Louis's extreme and equal accuracy of observation in respect to the 900 cases, and to carry more conclusively the imputed offence of suppression or misrepresentation. Thus:—"One would think that these facts were sufficient to enable one to come to some definite (we will not use 'conclusive,' as it offends our commentator so much) results. 'In my analysis,' *continues* Louis, '*I have wholly left out any facts which were not sufficiently exact*—and when I have deduced any consequences, I have always kept before me this idea by the author of Emile, I know that truth resides in things, &c.' In a note to this paragraph Louis informs us that he threw aside as incomplete all the 'observations' made during his first eight months of *devotion* to these studies. One would think that the *accurate examination* of about 1000 cases ['nearly 900'], and the autopsies of 1–10 of them, would have enabled any accurate observer to decide whether a lesion was *unimportant* or not.

"*So much* for Louis's *data* and accuracy of observation of nature."

Here the strong intent of Dr. B. to misrepresent me, and to make it appear that M. Louis bestowed as much personal care upon the whole

900 cases, and intended the whole to form equally the basis of his work, as the 138 cases of the typhoid affection, is again forcibly shown. And, to make the deception more effective, my critic introduces into the last quotation an extract from M. Louis, in which he (M. Louis) says "in *my analysis* I have *wholly left out* any facts which were not sufficiently exact,"—Dr. B. meaning to imply by this citation, in its connection with the whole paragraph and the preceding, that M. Louis *equally* employed in his "analysis" the entire 900 cases. And, to make this still more forcible, he refers to a note by M. Louis in which he (M. Louis) threw aside certain other cases. Now in this note, M. Louis adverts specifically to the 900 cases, and (without invalidating those), he does not limit the rejected ones, as Dr. B. implies, to "his first eight months," but leaves it indefinite. Thus:—"*Among those* which I did not consider sufficiently accurate, were all those which I collected during the first eight months of the six years," &c.

But the foregoing intent is rendered more conspicuous by the artificial and emphatic translation of the words—"In my analysis I have *wholly left out* any facts which *were not* sufficiently exact."

Now, what was my critic's first translation?

"In these analyses *I have not made use* of any records of disease which *did not appear* to me to be sufficiently exact."

And, what the French?—"J'ai retranché des matériaux de mes analyses les faits qui m'ont paru manquer d'un certain degré d'exactitude."

There are some minor circumstances attending the management of the foregoing new translation which go to show its extensive bearing, and the ingenuity with which it is executed. The reader will doubtless observe them; but I will detain him by indicating one which has a bearing upon a subject already examined. It relates to the question (so unimportant in principle) whether M. Louis had the actual treatment of the typhoid patients. It should be premised that I have observed the punctuation as it occurs in all the extracts which relate to the subject under investigation; and here, by the way, it will be seen that the new punctuation itself is sometimes designed to coöperate with the altered version.

Now, where it is announced by M. Louis, that Chomel had the supervision of the hospital, Dr. B. translates the French in the manner already stated, but which I shall here repeat for the convenience of the reader.

Old Translation.	*New Translation.*
"I collected between the years 1822 and 1827, the histories of all the patients suffering from acute diseases, which were admitted into the hospital of La Charité, in the wards St. John and St. Joseph, at that time under the superintendence of M. Chomel."	"*I examined and recorded*, between the years of 1822 and 1827, the histories of *all the patients* affected with acute disease, that were admitted to the hospital of La Charité *in the apartments under the supervision* of Mons. Chomel."

All the Italics are mine, except "*all the patients*," which my critic introduced for his own use, as already shown. The reader will see that the words "*St. John and St. Joseph*" are expunged from the new translation, as these are of specific import, and show that those two wards were probably appropriated, more especially than others, to M. Louis. In the original, and in the discarded translation, "the superintendence" evidently refers to the hospital at large, whilst a distinction is implied as to two of the wards. In the "revised and altered" translation, however, that distinction is obliterated.

> "Thus, tho' from truth I haply err,
> And sacrifice my character,
> What man of taste my right will doubt,
> To put things in, or leave them out?
> 'Tis more than right, it is a duty
> If we consider landscape beauty.
> He ne'er will as an artist shine,
> Who copies nature line by line;
> Whoe'er from nature takes a view
> Must copy and improve it too:
> Thus I (which few, I think, can boast)
> *Have made a landscape of a Post.*"—"*Dr. Panglos.*"

Having already trespassed too far upon this week's Journal, I shall defer my remaining examination of this involved subject. At the close of my fourth number, will be also found some remarks having an important relation to it.

No. 3.

The most important objects of Dr. B., as investigated in my last number (imputing misrepresentation to me, and placing the 900 cases on an equality), pervade his attack. Both are fundamental points, and are urged to show a general defect in my conclusions from their neglect of a primary element of my author's work. The reader is also thus diverted from the main object of my Essay, which is to show the pathological and therapeutical doctrines of my author, the amount and nature of the anatomical investigations, and how far these investigations were made the foundation of important pathological inductions. With these last I was especially concerned, and it was my object to prove that these were mainly founded upon the anatomical results of 133 dissections, of which 50 were cases of the typhoid affection. There were, however, certain cutaneous phenomena, meteorism, and all that related to fixing the typhoid disease at its incipient stage upon Peyer's glands, which involved the 88 cases of the disease that terminated in health. And, as those phenomena gave rise, respectively, to important generalizations, it was all carefully stated in my Essay. To appropriate these last generalizations to the typhoid affection, it was necessary to compare the cases with the vital phenomena of other diseases; and the residue of the 900 cases forms the ground of that comparison. This is the *full extent* of the importance they can possess under any circumstances; nor have the therapeutical conclusions the most remote relation to them.

But, Dr. B.'s perversion even exhibits me as having perpetrated the absurdity of supposing that M. Louis, whom I represent as having passed much of his life in hospitals and the dead-house, did not possess the means of comparing his 138 cases of typhoid fever with other diseases, and that I had not specifically admitted the fact. Nevertheless, I objected to the assumption of a greater number than the 138 and 83 cases, partly upon the ground of M. Louis's violent rejection of cases by others which are not accompanied by more substantial proof than his own, but mostly because I have shown that his pathological inductions, with the foregoing exceptions, are founded upon "the debris of the body." I have expressed no doubt of the vast extent of M. Louis's observation, especially at that time, in hospital practice. It would have been absurd to have questioned a fact so notorious. And though the great experience of my author required no admission to that effect from me, I regarded the results of that experience as exhibited in his works. I attempted to show, therefore, that he was not an observer of nature, but of the ruins of nature, and that his pathological doctrines are founded mainly upon the latter. I did not think this the *sort of nature* which, alone, makes a good book of "laws" and generalizations in pathology. I fully admitted my author's detail of vital phenomena, but I also endeavored to show that they were not rendered practical elements in his pathological data. A primary object of this, was to supply a practical demonstration of the validity of what I have said in my Essay on the "Comparative Merits of the Hippocratic and Anatomical Schools" as to the errors of the *exclusively* anatomical. I was concerned about his *book*, its doctrines and their ostensible ground; and my objection to any of his cases was not predicated of any real distrust that he had put forth what was not in itself substantial, but of his own requisitions and habits in relation to others. This I stated, that it might pass for what it was worth.

It being, then, my duty to give a fair representation of my author's work as it is, I laid out my ground, plainly, fairly, and fully; and, in so doing, I allowed the whole extent of any experience which it might be supposed that M. Louis had employed; though fidelity to my task compelled me to place the true basis of my author's generalizations in pathology and therapeutics in its proper light. At the opening of my Essay, I state that, "our author has occupied the proud elevation of presiding over the modern anatomical school; and it becomes, therefore, a matter of deep interest to inquire, through his remarkable labors, into the *practical* results of morbid anatomy, and how far it may have advanced, or retarded, the progress of medicine. The present Essay, therefore, may be considered *so far* a continuation of the last,"—or the Essay on the Hippocratic and Anatomical Schools. It became me to show that his generalizations in pathology were founded upon the debris of 133 subjects, the greatest amount that is stated by my author. This I have done, and most extensively, in his own words. And, although an analysis of only 54 cadavers is given, I allowed the whole that was claimed, and even all the subjects (88) that recovered of typhus fever, but whose cases, also, are not analyzed according to M. Louis's principles. Doubtless they had been; "but he should have given us the proof." This is his doctrine with past observers. To all the foregoing, however, I made no objection, as they entered, unlike the residue, into my author's most important conclusions,—especially the 83 dissections in divers maladies. But, did I not do more than this,—aye, even more than allow the 900 cases, with the qualifications I have mentioned? Did I not finally release M. Louis from the restraint to which he was subjected by a fundamental rule of the "numerical method," and to the observance of which he had laid himself under a deep obligation?

But, I must speak by the book; for doubtless my critic would otherwise deny that there is a word upon the subject beyond what he has presented in the mutilated extract; whilst, also, in so doing I shall protect his veracity. I will take for an example a statement which appears at the very opening of the Essay. After saying "it would be *unimportant* whether our author was the *acting* attendant, since he makes the cases entirely his own, erects generalizations upon them, and defends the whole treatment," I go on immediately thus:—

"And here it is important to bear in mind, that all our author's conclusions as to this affection were founded upon '138 observations of typhus fever, 50 of which were relative to individuals who died of it;' and, that he 'compared' this exact number of 50 cases 'with the alterations found as consequences of other acute diseases, in 83 subjects whose histories I learned.' Upon these cases, and *the lesions observed in other diseases*, our author founds those conclusions which will appear in the sequel."—(P. 685.)

Here, then, by the words "*lesions observed in other diseases*," as well as by the statement of 138 cases, Dr. B. is farther rebuked, and truth avenged. And then I went on thus:—"In the mean time, let us observe, also, in relation to the Typhoid Affection, that it is stated by our author, that—

"'In these analyses I have not made use of any records of disease which did not appear to me to be sufficiently exact, and whenever I have made deductions from those which were exact, I always kept in mind the thought of the author of Emile. 'I know that truth lies in the facts, and not in the mind that judges of them, and that the less I introduce what is merely my own into the deductions I make from them, the more certain I shall be of approaching the truth.'"

Thus, my reader had before him my author's premises and intentions; and, what I believe is not often practised, I sought for them in the "Advertisement," where alone they are announced. I have also given him, everywhere, the advantage of stating, in his own language, his various objects, all his "rigorous conclusions," "laws," &c., and all their contradictions; and it is this fairness, beyond doubt, which has most annoyed my critic. Next to that is the proof which goes to show the foundation of my author's pathological generalizations upon morbid anatomy. This was the fundamental purpose of my Essay, and without it, it could have had no existence. And next to that, Dr. B. is discontented with my demonstration, that all my author's particularities about symptoms, and all his cases beyond those of mere morbid anatomy, do not enter into his important generalizations in pathology. They are a mere show of words, and it was a special object with me to prove them such. My readers will find the proof by turning to the Essay itself.

But, let us have an example of my manner of objecting to a greater proportion of the 900 cases. After stating for the second time, the premises which relate specifically to the typhoid affection, I went on thus:—"Although the inductions are avowedly founded upon the foregoing number of cases, we are disposed to allow *any latitude of observation* which it may be supposed our author can have made. Nevertheless, it would be more correct to apply to our author the rule by which he restricts all others, and of which the following is one of the numerous instances of its derogatory insinuations:—

"'Corvisart does not state *how he proves* the truth of the assertion which he *tries to explain*. The reader may seek for the proof in his work, but where will the proof be found? It may be answered to this, that Corvisart had not observed those cases merely, *the histories of which he has published*, but that *his assertions* rest upon a *much larger number*. But, even in this case, Corvisart should have counted, and if he had done so, he would have stated the fact, since the question was simply one of number.'"—(P. 693.)

Here, too, it is evident, that the statement of the number alone would have only protected Corvisart against an impeachment of his veracity; since my author would have rejected, as is implied by the extract, and according to his well-known rule, so little observed in his treatise on Bloodletting as well as on the Typhoid Fever, all the cases that did not bear the "*proof*" of a critical analysis. In this respect he is imitated by our translator, who says he is "unwilling to take the assertion of any man"!—(P. 75.) This remark terminates the paragraph in which it is said that "Dr. Paine kindly consents to take Dr. Hale's assertion for the truth of the results of 197 cases,"—my true remark being intended to discourage this want of professional confidence. I was speaking, in a note, of Dr. Hale's objections to Perry's 4000 cases of typhus fever, in which he made 300 anatomical inspections. Now what does Dr. Hale do in this instance? Certainly, what I have not done in relation to M. Louis, as has been just seen by an extract from my Essay. Perry does all that Dr. Bowditch would require of M. Louis. That is, he states the number of his cases, 4000, and the dissections, 300, and gives us the result. These, too, were cases of typhus fever, whilst those which M. Louis announces in a round number were made up of various diseases.

Nor can I permit my objector to represent me as trifling with Dr. Hale. I must, therefore, bring the misrepresented statement before the reader. These are my words:—"It is objected (by Dr. Hale) that the '*particulars* of Perry's 4000 cases, and 300 inspections, are not given so as to enable us to judge how far he might be influenced in his observations by theoretical views, or with what degree of care and thoroughness his observations were made.' We would not have objected to this from M. Louis, but, whilst Dr. Hale gives us an analysis of only four of his 197 cases, we confide most implicitly in his general statement as to the remaining 193."—(P. 690.) Now Dr. Hale is justly esteemed an ornament of the medical profession. He inclines to the numerical school, so far as figures and analytical proof are concerned. He rejects Perry's 4000 cases and 300 dissections, because "the *particulars* are not given," &c. We might well, therefore, take his authority as to whether a greater part of M. Louis's 900 cases are entitled, upon the principles of the school, to any greater consideration,—and this more especially as M. Louis had in contemplation a multitude of "*theoretical views.*"

And, what says Dr. Bowditch, who is "unwilling to take the assertion of any man"? "The numeralists," he says, "*must have an analysis of facts recorded as they actually occurred*, and *at the time of their occurrence*, or they will not be satisfied."—(P. 76. *My Italics.*) This lets us into one of the motives for the new translation of the long paragraph, which was examined in my second number; and, doubtless, my reader has anticipated me in another, which I shall, however, now indicate. The surreptitious translation is a clear proof that Dr. Bowditch does not consider any of the cases beyond the 138 of the typhoid affection entitled to the consideration which he professes should be ascribed to them; otherwise he would have never fabricated so many words that are expressly intended to give the residue an equal importance, according to the new method. The fabrications, therefore, which are introduced into the new translation of the fundamental statement, completely divest all but a few of the 900 cases of any authority; according, also, to my translator's object and avowed doctrine. And that Dr. B. truly believes with me that the pathological and therapeutical inductions of M. Louis are founded upon the 138 cases of typhoid fever, appears from a declaration which he subsequently makes, and by which he restricts the whole anatomical premises to the 50 fatal cases of that disease. Thus:—"But, at present, we must conclude, both from Chomel's and Louis's researches (82 fatal cases collected during a space of 12 years by two eminent men), that the anatomical characteristics of typhus in Paris are as have been described."—(P. 90.) *Fifty* of these 82 cases go to form the substantial part of M. Louis's pathological inductions as to the typhoid affection.

I am now prepared to affirm, what I did not in my Essay, that the inductions of M. Louis, pathological and therapeutical, in his work on the Typhoid Affection, are virtually founded on 138 cases of that disease, and upon no other, except in a negative sense which relates to an experience of which we have not a "proof" in conformity with the doctrine of the author.

Having now unravelled a snarl which it was evidently supposed must remain untouched, from the extent of explanation which would be necessary; having made out fully all my specifications, and the ends of justice being substantially answered, truth avenged, and my critic disarmed, it may be thought that generosity should arrest my pen. It faulters, indeed, as the allusion escapes; and here should it pause, but that the fabrication, whose analysis I began in my second number, is made the substratum of most of the misrepresentations which compose the details of my critic's article. There is nowhere any attempt to controvert, with the usual honesty of reviewers, my facts and my arguments; but it is one unbroken series of defamatory charges, and ribald derision of my work. My adversary, therefore, lacking so fundamentally, and being destitute of the redeeming, though pretended, virtue of shielding another's fame, it has appeared to me that the whole force of his assault should recoil upon himself.

No. 4.

Dr. Bowditch states, that,—

"Dr. Paine never suffers his reader to lose sight of the *main object of his two volumes*, viz., a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) This is repeated, thus,—"the numerical (or anatomical, according to Dr. P.) school."—(P. 74.) And yet again:—"It would seem, then, that our commentator raised up nothing but a spirit; and we find him fighting *as a fundamental point* of the numerical method, a chimera of his own brain. This *two-fold error* of supposing the numerical and 'anatomical' schools *identical*, and that the *former trusts* to pathological anatomy as the ground-work of its system, *runs through* the whole of the hundred pages of criticisms."—(P. 79. *The Italics are mine.)*

I know not how to characterize these atrocious misrepresentations. I would most willingly avoid all offensive epithets; and it is only the distinctness with which I shall show their desert that I can offer as a justification.

My work, in a general sense, aims at very different, and far higher objects. It is extensively concerned about nearly all the great subjects in physiology, pathology, and therapeutics; and a *practical bearing* is everywhere given to the whole. Its general scope is to inquire into the prevailing philosophy in respect to each,—to indicate the methods of inquiry which should be observed by philosophers,—to exhibit the evils of hypothesis and the advantages of true theory,—to analyze our existing knowledge in the various branches examined,—and to wrest our facts from the grasp of speculation and reduce them to fundamental principles. Such are the objects of my work, and whether accomplished or not, it equally stamps the foregoing representations of my critic.

Let us now look at the statements more specifically. Dr. Bowditch presents me before the world, and before my work is known to the public, as having been guilty of the absurdity and injustice of "calling the numerical, the anatomical school," and of making them "*identical.*"

To enforce the intended effect, he also affirms that it is the "main object of the two volumes" to carry on "a violent attack upon the numerical, or, as he chooses to call it, the anatomical school."—(P. 73.) Now, in the first place, I have devoted an essay, of 37 pages, to a consideration of the "Comparative Merits of the Hippocratic and Anatomical Schools." Here, then, if anywhere, this imputed confusion should appear. There is not, however, in this Essay, a remote allusion to the numerical school. That school forms a special, but brief subject of consideration in the Essay which is devoted to the "Writings of M. Louis," and in my "Philosophy of Bloodletting." Morbid anatomy I have everywhere advocated, and have labored to point out its legitimate and important relations to pathology; but always holding it in subordination to the vital signs. The numerical school, *in M. Louis's acceptation*, I have condemned with as little ceremony as he has all preceding observation; nor have I any apprehensions of my success in this respect. It will be thus seen that Dr. B. has a comprehensive purpose in the foregoing statement. It represents me as deficient in common understanding,—my work as hostile to morbid anatomy as to "numeralism," and without that element in its foundation. A distinction is even cautiously maintained between the anatomical and numerical schools, which have no other than an incidental connection. At the beginning of my Essay on the Writings of M. Louis (p. 683), after saying that its principal object is to exhibit "the practical results of morbid anatomy," and that it "may therefore be considered so far a continuation of the last" (or the Essay on the Hippocratic and Anatomical Schools), I add, that, "we have also bestowed *some notice* in our first volume upon our author's *numerical system* in its relation to bloodletting; but, we shall *transiently notice it again in other aspects*."

Again, however, there is no incompatibility between the two schools, whatever may be the abstract distinctions. The "numerical" may take the lead in the anatomical, and embarrass it with absurdities. But this only proves their elementary distinction. Bichat and Hunter were general anatomists of the highest consideration, each variously dissecting, but always rendering necroscopic results subordinate to vital phenomena. Therefore is it, that comparatively little of the former appear in their writings, especially those of Hunter. Their pages glow with nature as she glows, and they have but little of the odor in which the strict anatomist delights. The dross of the dead-house is subjected to an alembic, and a sweet distillation imparts its delightful perfume. Hence I have said in my work, that,—"Coming to Hunter, we find him analyzing the principle of life, and expounding the whole philosophy of inflammation, with scarce a reference to a post-mortem examination" *(Vol.* 2, p. 674); whilst, in another place I have it,—"who was neither *anatomist*, physiologist, surgeon, nor naturalist alone, but the most remarkable combination of *all these* that the world has yet seen."—*(Ibid.*, p. 588.) All this is exactly true. Of Bichat, I have said,—"It is not, then, the *great architect* of the *anatomico-pathological* school, *of which we are a humble advocate*, who laid the foundation wrong; but the glare of his light was too much for his countrymen, who, as Armstrong expresses it, 'have just burst from the old system of pathology,' and who, as avowed by Travers, are unacquainted with the achievements of Hunter."—*(Ibid.*, p. 663.) My opposition related to the "excesses of the dead-house" (p. 663), not to morbid anatomy as cultivated by Hunter and Bichat. But, morbid anatomists as they were, think you that either would have tolerated "*the* numerical school"? Andral and Louis are placed, by common consent, at the head of the present anatomical school, whilst Broussais is ranked as a chief by a reviewer with whom I had the honor of an argument.—(Vol. 2, p. 645.) But, would Andral esteem it a compliment to be classed with the "numeralists," and did not my critic fear that the ghost of Broussais might call upon him for atonement?

Again, Dr. B. is pleased to say that "there is scarcely a hundred successive pages in either volume, in which this opposition does not manifest itself,"—that is to say, "a violent attack upon the numerical school,"—"the main object of the two volumes." Now there is no allusion to the subject, unless very incidentally (and of such I have no recollection), in any one of my Essays, excepting in those upon Bloodletting and the Writings of M. Louis. In the former, a short space only is devoted to its consideration, and I only recurred to it briefly in the latter. Here, in this Essay on the Writings of M. Louis, Dr. B. gives to his reader the *impression* that I have "dedicated a whole chapter of 134 pages" to exploding the "bigoted numeralists."* Now it happens that I have "dedicated" only three pages of this Essay to that object, and these are *isolated* from the rest by a black line drawn at their commencement and termination, that the individuality of this part of my subject might be at once obvious to the most careless observer. These three pages begin with the following sentence. Thus:—"Of the *numerical* method we have, perhaps, said as much as is incumbent upon us in our first volume, pages 293—309, 332. We have there exhibited an instance of its practical application in the hands of others (p. 305), and we will now present an illustration by our author."—(P. 780.) Indeed, Dr. B. has unwittingly indicated the amount of attention which I have given to the numerical school. Thus:—"Our author progresses in his zeal, and devotes *three* pages (Vol. 1, p. 293) to the improprieties of the numerical school" (p. 76), and just this amount, as I have shown, is given to the subject in my article upon the Writings of M. Louis. This Essay is mainly taken up in indicating the abuses of morbid anatomy,—in showing the generalizations of my author which he founded upon them,—in pointing out the dangers of false philosophy,—in arraigning my author for a departure from his own rules in philosophizing,—in protecting my brethren, from Hippocrates to the present time, against his almost universal scorn and derision,—in demonstrating the fallacy of the assumption that "medicine is now in its infancy."

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* Pp. 73, 74. Dr. B. quotes these two last words with the manifest intention of imputing the epithet to me. Not having had the leisure to look over my work, I will not positively affirm that such an expression may not have escaped me. I believe, however, that the word "bigoted" is not in the book. The use of the word "strangled," in connection with bloodletting, is also imputed to me. (P. 75.) It is possible I may have committed the barbarism; but if so, I call upon Dr. B. to publish the remark, as well, also, that in which the words "bigoted numeralists" are said (by marks of quotation) to have occurred—the volume, and pages. If I am convicted, I fully allow the justice of the criticism. If I am not,* * *

The foregoing misrepresentation is also extended to the anatomical school. Now this school does not come under consideration in my first volume, excepting where I speak, in my Essay on Bloodletting, of the numerical method, and where, also, I bring forward morbid anatomy to *sustain* my argument with Dr. Hall as to the imputed effects of excessive bloodletting. And here, by the way, the few pages which I have devoted to the *numerical* school are also isolated from the section by a line. The subject, too, as in the instances already cited, begins with a statement which makes a radical distinction between the numerical and anatomical schools. Thus:—"We shall now proceed to an examination of the treatment of simple pneumonia, and of some other inflammatory affections as founded upon the 'numerical method' in *its connection* with morbid anatomy *as cultivated* by the *exclusive* philosophers of the *anatomical* school."—(Vol. 1, p. 293.) It will be recollected, too, that Dr. B. refers to this very page, and had he fairly presented the subject in the foregoing acceptation, and called it *Louism*, I had not objected.

Coming to the second volume, I have nothing on the subject of the anatomical school in my Essays on Animal Heat, the Philosophy of Digestion, and the Theories of Inflammation; unless in the last indirectly, and not in the sense imputed by my reviewer. In my Philosophy of Venous Congestion, there is nothing upon the anatomical school, unless a casual reference, and here I bring up morbid anatomy, *and far more extensively than has been allowed*, as an *auxiliary* to my pathology of that disease. Finally, it is not, till near the end of the second volume, where my article on the Schools is placed, that the question is brought forward. In my Philosophy of Venous Congestion, a remark occurs which farther places this subject and my critic in their proper attitude. Thus:—"In our Essay upon the Comparative Merits of the Hippocratic and Anatomical Schools, and in our examination of the Writings of M. Louis, we have endeavored to show the superiority of the vital signs in marking the true pathology of disease."—(P. 316.)

Now, as to the second part of the "two-fold error," "that the *numerical* school trusts to pathological anatomy as the ground-work of its system." If M. Louis be assumed as *constituting* "the numerical school," then is the affirmation correct; otherwise it has no foundation. No one venerates more than myself the philosophical habits and labors of many (as Jackson, Hale, Gerhard, &c.) who employ the numerical method as an *auxiliary* to medical science. This remark leads me to say that I have been misapprehended in my views of the method, and perhaps I may not have been sufficiently explicit. Now it so happens that *I like the method* without its abuses, and as such have long employed it. Whenever I have spoken of it, I have always intended M. Louis's "numerical method," which rejects all observation that is not founded upon it, brings pathology and therapeutics under the dominion of mathematics, regards not the various considerations which relate to climate, constitution, habits, age, sex, &c., and practically knows little else than a *balance sheet*. As the method existed prior to this innovation, it offers important advantages as a general memorandum. It is of

M. Louis's "method" and "numerical school," therefore, that I have spoken in my work. I would advocate the old method, or, as Dr. B. has it, "the plan originally proposed by others," and "we hereby give in our faith."—(P. 78.)

Dr. Bowditch brings me forward as the champion of Chomel. "Dr. Paine," he says, "seems to think himself called upon to defend the reputation of Chomel," &c. Then follows a long exposition, implying that I have conveyed an impression that Chomel undervalues his friend Louis. The latter implication is without a shadow of foundation; and, as to the former, its only source is my remark that, "this distinguished observer, however, should be in no respect associated with our author's performance" (p. 685), and this, as will soon be seen, was modified immediately afterwards. Although I have now stated the whole of the "defence," Dr. B. nevertheless has it—"as our author thought it necessary to devote *two pages* to the subject, we were unwilling to pass it by unnoticed."—(P. 79. *My Italics.)* It is certainly painful to me to call up subjects of this nature, unless for the purpose of some general interests. Yet, being required to meet a calumniator, I must show the reader with what reference to truth I have been presented as the "defender" of Chomel. This will appear farther from the following extract from my work. Thus:—"And, *coming to the writings of M. Louis*, we shall make it a particular object to inquire how far both himself and *Chomel* are entitled to the rank which is awarded to them by the able writer who stands at the head of this Essay, as we have already endeavored to show with what consideration 'M. Andral is allowed by the profession to be the first pathologist in the world.'"—(Vol. 2, p. 645.) And now as to the charge of having placed Chomel in opposition to his friend Louis (p. 79), take the following passage, from my Essay:—"Not only such an example, but a multitude of them, occur in the writings of an author who will *not be suspected of any disposition to interfere with M. Louis's generalizations.* This author is Chomel, *so largely interested in the works on Typhus, and Phthisis, now under consideration.*"—(P. 687.)

This last quotation carries us back to my 1st No., as it bears directly upon Dr. B.'s statement of my denial of Chomel's "superintendence" of the hospital. The remark occurs, also, within two pages of my general statement of the fact, as expressed in the "Advertisement," and of the misrepresentations which I am now investigating.

In reference to the controverted anatomical lesion of Peyer's glands, my commentator affirms, that,—"Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel;" and, to carry out this deception (and the memorable one connected with it, as shown in my second number), Dr. B. troubles the subject at great length. (P. 81.) Now, although I have introduced Chomel as the first of my witnesses because he was the *earliest* in order as to time, it will be seen by referring to my Essay that he is one of the *least* in importance; nor have I brought his testimony as having been arrayed (according to Dr. B.) against M. Louis's conclusions. On the contrary, it is immediately preceded by the last quotation from my work (p. 687).

I have, also, even invalidated his statements with the "numerical school," by saying that his "facts are rather *old*, but little known, and more frequently neglected." I cannot, therefore, consent that my greater witnesses, Tweedie, Lombard, Perry, Craigie, Alison, The British and Foreign Medical Review, Dunglison, Geddings, Hale, &c., should be crowded aside for the benefit of the master. It is owing to this large body of proof, clear and decisive, and the inferiority of Chomel's, that Dr. B. misrepresents the latter, that it may appear to follow that I have not aided in putting at rest a question upon which much of M. Louis's fame has rested. By turning to pages 688—692, the reader, who may have the curiosity, will see how the case stands between Dr. Bowditch and myself.

The perverted extracts which formed the subject of my second number are preceded by more than a page of introductory comment, which relates exclusively to the controverted anatomical lesion of Peyer's glands. The reader is here prepared for the coming event by imputing to me an unfair representation of Chomel's opinion of that lesion. This charge is conveyed in a manner so well introductory to a succession of deliberate acts of the very nature of the offence which they fabricate and condemn, that the reader will be gratified with seeing it. Here it is:— "The reader doubtless will suppose, from what we have extracted from Dr. P.'s remarks, that Chomel believes that the peculiar lesion of the intestinal follicles ascribed by Louis to the typhoid affection can be found in many diseases. Now we deny that Chomel ever said so, or meant to be understood so to say; and we assert that he declares exactly the contrary, and that it is Dr. Paine's *garbled quotation* that has led the reader into error."—(P. 82. *My Italics.*) I cannot encumber my reply with extended quotations from Chomel to show my translator's misrepresentation. But, as it was designed mainly as a *ruse* to give effect to the greater fraud which followed, I shall now state all that occurs, in my work, upon this subject in relation to Chomel, and of which Dr. B. predicates his affirmation that,—"Among the ablest and worthiest, and the one upon whom Dr. Paine rests his greatest hopes, is Chomel."— Thus:—

"Chomel states that alterations of the glands of Peyer were common in the epidemic Parisian cholera of 1832; so common, indeed, that he was disposed to carry M. Louis's philosophy of the dependence of typhus upon the glandular affection to the cholera itself, and to establish an *affinity*, if not the identity, of these diseases. [*This is true.*] It is certain, that such had become the ascendancy of the glands of Peyer in 'pathological anatomy,' that, as in the typhoid affections, those glands were regarded by the sçavans of Paris not only as the veritable *seat* of cholera, but as the cause of its morbid phenomena. [*And this is true.*] Thus:—'On se rappelle que, dans l'épidémie de 1832, les premiers observateurs qui eurent l'occasion d'ouvrir des corps de cholériques crurent avoir trouvé dans la lésion des follicules intestinaux, et *le siège* de la maladie, et *la cause* de la plupart *des phénomènes* morbides qui la caractérisent.' But, this is not all which Chomel supplies. He has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina, and other affections (sujets morts d'affections differentes); with the *exception of the ulceration.* [*And this is true.*] He states one case, in erysipelas of the face."*—(P. 688.) *This is also true*, and the end of my "ablest witness"! The reader will also not fail to observe, in the first part of the extract, how far I had placed Chomel in hostility to Louis.

The reader, by referring to Chomel's work, will see that there is no "garbled quotation," no words *fabricated*, or *expunged* from the quotation, and that my author's statement is fairly expressed. Nor should I neglect saying, that it was my principal object, in quoting Chomel, to show, through him, what had been the opinion of the "sçavans of Paris" as to the dependence of *Cholera* upon the follicular lesion. This is my great proof from Chomel, and it is undoubtedly important.

It will be seen, also, that I have stated the only exception of the least importance (that of "*the* ulceration"), as it respects Chomel's own cases, that the reader might judge for himself, whether the follicular lesion observed by Chomel in "other affections" than the typhoid, was not so nearly allied to that which had been observed by M. Louis in the latter disease, as to divest the typhoid affection of its imputed characteristic. This was my own opinion; but I did not say, as alleged by Dr. B., that it was the opinion of Chomel. I was merely stating a fact; and if any one thing, more than another, can show the trifling with morbid anatomy, and that it is made the basis of pathology by Louis, Chomel, and Dr. Bowditch, it is the very pottering about this lesion of Peyer's glands, and whether, as to Louis, at least, it shall be the *seat* and *source* of all the structural lesions and all the vital phenomena in the typhoid affection, when it may be *ulcerated*, or appertain to some other disease when it is *not ulcerated*, but having essentially all the other morbid attributes.—*See B.'s attack, p.* 82.

But, I have a few words more as to this charge of falsely representing the facts from Chomel. This is predicated of a fraudulent extract from my Essay. I shall place the right and the wrong ones in parallel columns.

My Remark.	*Dr. Bowditch's Quotation.*
"Chomel has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections differentes), *with the exception of* THE *ulceration.*"	"Dr. Paine says that Chomel 'has seen the same alteration of the glands of Peyer as attends typhus, in scarlatina and other affections (sujets morts d'affections differentes).'"

The reader will see that the important qualification is *expunged;* and having committed this act, Dr. B. proceeds *immediately* to say, "We are sorry to see such a lamentable deficiency in the fairness which we expect in one who *quotes*"!—(P. 82.) This was a fitting preliminary to the other mutilated quotation, and the false translation, which follow in immediate connection, and which were the subject of remark in my second number.

A fundamental anatomical question will begin my next number.

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* Lecons Clinique Medicale, pp. 171, 178. 1834.

No. 5.

Dr. Bowditch "denies entirely the truth of the assertion, that Louis or the numerical school do make pathological anatomy a *paramount* guide in pathological inquiries; and Dr. P. must know little of the matter when he accuses Louis of thus using it."—(P. 78.)

Here the reader will see an involution which has been more directly presented in other places, viz., that of making me extend my premises which relate alone to M. Louis, to the whole "numerical school," and thus, also, to maintain the imputation of confounding the anatomical with the numerical school. The statement, however, falls, of course, under the denomination of all the others. But, let me in the first place say, that it may be far more correctly affirmed of the anatomical than of the numerical school, that the former does make "pathological anatomy a *paramount* guide in pathological inquiries." This is notorious the world over; and it is so emphatically true of M. Louis, that nothing evinces the assurance and folly of Dr. B. more clearly than its denial. It was the great object of my Essay on the Hippocratic and Anatomical Schools to show the advantages of the former in making anatomy subservient to the vital phenomena of disease, and to indicate the evils which have resulted from the opposite system, and from which the latter school derives its very name. In respect to M. Louis, I had mainly in view, as I have already said, a *practical illustration* of the sad results of "making pathological anatomy a *paramount* guide in pathological inquiries;" and as to his "numerical method," I was scarcely concerned (and only in three pages) about that stupid affair. "It is as mechanical," says M. Double, "as the employment of a shoemaker."

But, let us have something directly to the point,—something from the *master* himself, be he anatomist or numeralist. In the work on Phthisis, Louis thus lays down the doctrine:—

"We have divided our work into two parts, and since ANATOMY IS THE STRONGEST SUPPORT OF PATHOLOGY, we have *commenced* the analysis of our facts by a general description of the *visceral lesions*."—(*Preface, p.* 70. *My capitals and Italics.*)

I have also copied the foregoing declaration from Dr. Bowditch's "revised and altered" edition. Again, practically:—

"We must infer that it is in this last lesion (the glands of Peyer), and *not in any other*, that we must look for the cause of the *delirium*, and more especially of the *somnolency*," in typhoid fever.—(*On Typhoid Fever, Vol.* 2, *p.* 132.)

The foregoing anatomical doctrine runs through the works on Phthisis and on the Typhoid Fever. In the Essay devoted to the writings of M. Louis are hundreds of extracts setting forth his generalizations which were founded upon the "debris of the body." No small part of Europe has been long at war with him upon this subject. The contest began very early, and that distinguished philosopher, Dr. Jackson, wrote several years ago, after the following manner:—"It is objected by some to the labors of M. Louis, and others of the French pathologists, that they labor, indeed, with ardor on the subject of *diagnosis*, that they study with the zeal of entomologists to *discriminate minute changes of structure* in the various textures of the human body, but that they *do nothing* to advance the proper business of the physician, the art of healing. Their therapeutics are decried, as showing an ignorance of what has been thought certain in England and in this country; and they themselves are regarded even as indifferent to this branch of science."* *(My Italics.)* This was Armstrong's opinion, who says,—"They (the French) seem as if their only object was to find out the disease without any reference to its relief or cure." And how find it out? Hear Armstrong again:—"I trust the facts of the French, but I receive them with great caution. They have just burst from the old system of pathology, and seem to me to draw *conclusions too general from a few facts.*" And have I not shown this, most extensively, in regard to M. Louis? What was Laennec's doctrine?—"Pathological anatomy," he says, "is incontestibly the surest guide of the physician, either in the *recognition* of maladies, or in the *cure* of those which are susceptible of cure." Andral may be said to personate morbid anatomy as much as M. Louis, and it is from the dead-house that he has exploded inflammation as a disease. Hear Mr. Travers:—"I am desirous," he says, "to guard myself from the imputation of being in the slightest degree *tainted* by the heresy of the French school, that there is *no such reality as inflammation;* that 'it is an old-fashioned coin, of which the impression is effaced, and that it ought now to be withdrawn from circulation.' *(Quoting Andral.)* If the study of diseased actions had gone hand in hand with that of diseased structures, it is impossible that such a doctrine could ever have been advanced. Out of the debris of the dead subject, however accurately inspected, examined and arranged, to attempt a solution of the great problem of living actions, and to build upon such a foundation an edifice of pathology of self-support, is as injurious a fallacy, and scarcely less arrogant or absurd than that of the Cartesian Philosophers, who undertook, out of the depths of their anatomical sagacity—to make a man." "The effect of morbid anatomy holding the first, and only place in the mind of the medical inquirer, is to substitute effect for cause, the laws of physics for the laws of life, to confound the cause of death with the cause of disease, and in short to obscure by attempts at simplification."

This is what I have taught in my Essay upon the schools, where I have endeavored to give to morbid anatomy its legitimate importance. That importance will be gathered from the whole Essay, and not from an isolated remark. I have now quoted Mr. Travers for the purpose of exhibiting a perfect portrait of M. Louis's work on the Typhoid Affection. My Essay presents the original, from which the foregoing semblance may be supposed to have been taken in shadow. It is useless to talk of M. Louis's detail of symptoms. The most that he makes of them, is to assume them as indicative of the primary seat of the disease in a structural lesion of Peyer's glands, as set forth in my Essay at pages 716, 798, &c., whilst my author, as shown at p. 790—797, generally gathered a knowledge of the early symptoms from the patients themselves, or their no less squalid companions.—(See p. 773.) This is fully allowed by Dr. B., who turns aside its obvious application by the remark,—"Suppose Dr. Paine had been the observer,—how would he have learned about the previous history except from the patient and his friends?"—(P. 108.)

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* Jackson's Preface to Louis on Bloodletting, p. 25.

Now, in the first place, Dr. Paine objects to taking this class of patients (especially from Parisian hospitals), for the purpose of supplying any useful facts in pathology, and he has given reasons for it. Their constitutions are wretched, their treatment worse, and they are full of organic lesions before they enter the Gallic institutions. As Armstrong, Travers, &c. imply, they are then mostly looked upon as good subjects for the dissecting knife; nor is this ever lost sight of by my author. But, Dr. Bowditch's admission, or interrogatory, amounts to no justification. It leaves the subject just where I had placed it, and shows the absurdity of taking the preliminary symptoms as a true source of knowledge as to the primary seat of the typhoid affection, and, therefore, that my author was fundamentally wrong in putting forth a doctrine which has been at the foundation of his celebrity. It often happened, too, that my author could get no account of the preliminary symptoms.—(P. 790.) Nor should it be forgotten, in allowing my author so much credit for the Hippocratic mode of observing nature, that the typhoid patients were generally severely sick for many days before they took up quarters at La Charité, and were sometimes "*a little delirious*" when supplying the important details for the benefit of science.

Now, although I have made all the foregoing perfectly clear in my Essay, yet, as I can only lay certain parts of it before my reader, I will give him the *quod erat* in words of my author, which, on account of the clearness of my demonstration, I did not think proper to introduce into my work. I endeavored to avoid what might seem supererogatory. Thus, then, my author:—

"Physicians not much conversant with hospitals, or who seldom practise among the laboring classes, will not readily give credit to these remarks; but those differently situated are aware that, whether it be from indifference, or dislike to hospitals, patients *seldom enter until quite late; even when their diseases have been very violent from the beginning*."*

So much, then, as to tracing the primary seat of the Typhoid Affection to an anatomical lesion of Peyer's glands, and the value of such subjects for important scientific and practical purposes.

What I have said of my author's symptomatology, I have substantiated out of his own works, and this in all parts of my Essay. I remark that,—"Our author's philosophy is necessarily concerned about *symptoms*, so far as they are important in showing that the disease began in the intestinal canal, and were significant of a primary lesion of the glands of Peyer. This is of vital moment to the hypothesis as it respects the foundation of the disease, and of the entire superstructure."—(P. 790.) "All the rest reposes mainly upon the *color* of the parts, *red* and *white* being the standards, and as other shades may approach either of the foregoing, they are appropriated accordingly." "Our author's 'Remarks' upon every case consist especially in speculations about the various colors which were observed."—(P. 798.) This is true, and this is the test. His "Remarks," which go to form his pathological conclusions, are almost entirely concerned about the "debris of the body," with the exception of those preliminary symptoms which he got from the patients, their friends, or not at all. Nevertheless, I fully, and repeatedly allowed, in the course of my demonstration, that, "although our author does not take into account the vital signs in making up his principles and generalizations, he has supplied a *memorandum which others may employ*."—(P. 716.) So obvious have I rendered all this, that my able New-York Reviewer, of whom I formerly spoke, manfully takes the broad ground of Morbid Anatomy.

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* Louis on Bloodletting, p. 7.—*Putnam's Translation and Punctuation. Boston*, 1836. *My Italics.*

But, suppose it had been otherwise, and my author had taken the vital phenomena, along with the cadaverous decompositions, into his pathological conclusions, what were such subjects worth, and what the value of his generalizations? There is not one case, in my opinion, which was worthy of record, and this opinion I have placed deliberately before the world for what it is worth.

The foregoing subject will continue to be more or less under review, and I may now remark that if anything in the way of proof can bear upon a question, it is, that the fact of my author's having made all the anatomical lesions and all the symptoms, in the typhoid affection, to revolve about the structural lesion of Peyer's glands, proves, beyond controversy, that my author is a "*dead*-anatomist." But more of this hereafter; and, in the mean time let us hear Dr. Lewins as to the anatomical school:—"I have often thought," he says, "that our most eminent pathologists sometimes appear much more desirous of having an opportunity of dissecting the bodies of the dead, than anxious to make any practical application of their post-mortem examinations for the benefit of the living." Nor may it be amiss to quote M. Ribes in my behalf. Thus:—"*The time will come* when physicians will feel that they have been very justly censured (qui ils étaient mépris), for having thought that pathology consisted exclusively in the study of organic degradations, and that they merit the same reproaches which they have bestowed upon others for confiding in the symptoms alone."—*(Anat. Path., &c., Pref., p.* vii.*)* To whom does Dr. B. suppose that all the foregoing authors refer? This, however, is but that circumstantial proof which consists in universal opinion. I go back, therefore, to the more direct, and will have something more of the internal nature.

Dr. B. offers the alternative of choking me with a "falsehood," or with having read my author in a blundering manner. Not choosing to avail myself of either, it only remains for me, as hitherto, to impale him on both horns of his "dilemma." I protest, however, against all imputation of a proper want of generosity. My reader will yet see that no opportunity has been left for its exercise.

Dr. B. quotes me accurately in the following manner:—"Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure; and when he endeavors to *insinuate* the belief that diarrhœa cannot exist 'without *appreciable lesion* of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body."—(P. 695.)

My critic then remarks that,—"Our readers would scarcely believe us if we were merely to state that all this is radically *false*" (p. 91); *(my translator's Italics)*; and, after quoting and re-modelling the translation, proceeds to force an induction that I had really affirmed that M. Louis had positively denied, in this place, that "diarrhœa could exist without appreciable lesion of the mucous membrane." But, although I have stated exactly the fact in this particular instance, I have shown, from my author's work on the typhoid affection, that he does deny the independence of diarrhœa, in that complaint at least, of structural lesion.

I shall now present the whole paragraph which has been quoted by Dr. B., but not according to his new version. I quote from Cowan's translation of the work on Phthisis, "revised and altered" by Dr. Bowditch, and which I formerly employed. Thus:—

"'Let us remark, that these copious perspirations indicated disorder in the functions of the skin, as remarkable by its intensity as duration; that this disorder, whether sympathetic or otherwise, was not the less positive, and existed without any sensible change of structure in the organ itself; and that thus a function may be more or less modified during a long period of time, while the organ on which it depends offers no appreciable change of structure. We may also observe, that while facts are wanting to *prove* distinctly that diarrhœa may exist without appreciable lesion of the intestinal mucous membrane, *we may presume this to be the case, from the analogy existing between diarrhœa and more or less profuse perspiration. Of this we cannot be positive, for in our opinion, analogy is only useful to point out fresh subjects for investigation, to lead us to the discovery of facts*, but never to supply them. Were it otherwise we might conclude that a thing really existed because it was possible, which is absurd.'"*

The words in Italics embrace the question at issue,—the word "*prove*" being in Italics in the original but not in the new version; which shows the *quo animo* of my translator at the time of each "revision and alteration." In my work occurs the following quotation, preceded by—"He therefore says," which is left out by Dr. B. as well as the source of the quotation from my work. Thus:—"He therefore says,—

"'We may presume that it may exist without, from the analogy existing between diarrhœa and more or less profuse perspiration.' But, '*of this we cannot be positive*; for in our opinion analogy is *only useful to point out* fresh subjects for investigation, to lead us to the discovery of facts,' &c."—(P. 695.)

The first words in Italics are translated anew by Dr. B., "we do not say that this is proved." Now, taking this extract alone, my statement is fully made out to be perfectly just,—viz., that, "when he endeavors to *insinuate* the belief that diarrhœa cannot exist 'without appreciable lesion of the intestinal mucous membrane,' he fears that his hypothesis may find some opposition from analogies supplied by the natural conditions of the body." But, I might have made my case stronger, in my Essay, by quoting in this place the sentence immediately preceding, in which the word "*prove*" occurs in Italics. And, when the reader regards the little importance which is here attributed to analogy, and that the question is thus turned aside for *fresh investigation*, I should have drawn even a more "rigorous conclusion" than that my author had "*insinuated* the belief," which he carries out in his work on the typhoid fever,—whatever concessions he may have incidentally made in behalf of functional disease (as I remark in my Essay), for the apparent purpose of more effectually establishing the anatomical doctrine by the occasional, but very equivocal, manifestation of impartiality.—(Pp. 703, 704, 708, 716, 766, 790, &c.) Can anything have been fairer, however provoking?

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* On Phthisis, Sec. 259.

As to the affirmation, that, "Our author, for instance, has no conception of disease which he cannot trace out through some lesion of structure," that is of a general nature, as was well understood by Dr. B., and refers to a vast amount of proof which I subsequently offer to that effect. I was employed in the foregoing instance in showing how my author objected to analogy in the hands of others, and how well he could employ it himself; and, in the course of these remarks I stated, what it was a special object of this Essay to illustrate, that, "the whole of our author's stupendous fabric reposes upon morbid anatomy, and upon reasoning from the *dead* 'to the *sick* himself.'"—(P. 698.) It is the proof upon proof which I have offered, embracing a great part of the Essay, at which my critic is annoyed, and therefore hoped to show by the foregoing incidental quotation that M. Louis had been really misrepresented upon this fundamental point. It was one of my constant objects, also, as I have said, to exhibit my author's contradictions, and I have repeated comments upon the subject. It was one of my motives, in allowing by the foregoing quotation that M. Louis left it doubtful whether or not diarrhœa took place "without any appreciable lesion of the intestinal mucous membrane," to contrast the *doubt* with the *certainty* which is expressed in the following quotation. Thus:—

"'At this period, in fact [the *commencement*], the *elliptical patches* were, if not in all, at least in *nearly all* the cases, the only part of the canal which was diseased, and consequently, THE ONLY PART to which the DIARRHŒA *could* be *referred*.'"—(P. 758.)

Now, this looks to me, without my other voluminous proof, a good deal like denying that diarrhœa can arise from mere functional disease. This occurs where I am employed in showing by 35 quotations that my author refers all the lesions of structure, and all the symptoms, in the typhoid affection, even those of the brain,* more or less directly to the altered anatomical structure of Peyer's glands. But, if the foregoing do not satisfy my critic, let him take the following:—

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* M. Louis, after enforcing his anatomical doctrine, that all the symptoms and structural lesions, in the typhoid affection, should be referred directly to the anatomical lesion of Peyer's glands, but suppos-

"'Although the diarrhœa was *very great*, the mucous membrane of the colon presented *no evident traces* of *inflammation*, inasmuch as the thickening may be considered as the consequence of the *reaction occasioned by the meteorism*; so that the *alteration of the patches* of the small intestine was doubtless in a great measure *the cause* of the abundance of the alvine discharges.'"*—(P. 712.)

Here, then, are laid down several great principles. 1st, that inflammation, or "an appreciable lesion of the intestinal mucous membrane," *is necessary to diarrhœa.* 2d, that thickening of this membrane, unless it be *red* (which is meant by M. Louis, no matter how a "profuse diarrhœa" may overcome the redness), is not owing to inflammation. 3d, that such a thickening is a consequence of the reaction of the meteorism! at least "in a certain number of cases." 4th, that, in the typhoid affection there is a specific lesion of structure for the diarrhœa, namely, "a specific alteration" of Peyer's glands.—(See M. Louis, Vol. 2, p. 449.) 5th, that, in the typhoid fever, at least at its commencement, the diarrhœa proceeds from the altered structure of Peyer's glands. 6th, the whole induction, as to the primary seat of the disease (being a fundamental object of M. Louis's treatise), is shown by these extracts, as in numerous other instances, to have been founded on the vague information which he got from the squalid victims of a Parisian hospital, and who, according to my author's own statement, "*seldom enter until it is quite late; even when their diseases have been very violent from the beginning.*"

The structural lesion of Peyer's glands being the pivot about which the whole machinery revolves, I will here present another example, in which my author endeavors to strengthen his induction as to the dependence of the diarrhœa upon that lesion, and in which is involved one of his constant conclusions, either direct or indirect, that there can be no disease without a lesion of structure. It is also an example of the multitudinous instances in which he establishes "important laws" upon the "debris of the body," and shows how far Dr. B. has correctly informed his readers in contradicting my statement that "pathological anatomy" was mainly the ground of my author's pathological conclusions. I must, however, in the first place, quote my author's premises, which immediately precede the extract in question. Thus:—

"'Of all these lesions only one was constantly found, namely, an *alter-*

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ing that all may not concur in his views, is peremptory as to the cerebral. Thus:—"We must, moreover, remember that *if* the greater part of the symptoms are explained by the *state of the organs with which they are connected* [referring to the possible views of others], this is not the case with the *delirium, which we cannot explain by the apparent state of the brain*; [no anatomical doctrine this;] that, *more than any other symptom*, it seems *to be dependent* upon the small intestine, in the typhoid affection, so that it appears that *its treatment* ought not to differ from that of the *specific alteration* of the small intestine."—(*On Typhoid Fever, Vol. 2, p. 44*[illegible].) And thus, in my Essay,—"Nor should it be forgotten, that, in nearly all our author's fifty cases, there was either *injection*, *softening*, an *accumulation* of serum, or some other unnatural appearance in the brain. 'The pia mater,' says our author, 'was injected in a little less than half the cases'; 'the medullary substance of the cerebrum was more or less injected in all the cases excepting seven'; and the cerebellum was only 'more frequently in a *healthy* state than the cerebrum, and when it was *not so*, it generally was affected in a *similar manner* and in the same cases.'"—(P. 762.) So much for my author's morbid anatomy.

* My Italics, as they generally are when not otherwise indicated.

ation of the elliptical patches of the small intestine, to which may be added a morbid change in the mesenteric glands. I have considered it *as inseparable* from the disease we are now studying, and as absolutely forming its anatomical characteristic." " I must conclude *the lesion of the* elliptical patches *began at the commencement* of the disease. Although the *other lesions* must be considered *as merely accessory or consecutive*, still they commenced often quite soon after *the principal disease* "—[that is to say, the lesion of Peyer's glands]. " The anatomical characteristic of typhoid fever becomes still more manifest by the comparison of *the lesions* previously given, with those presented by individuals who died in consequence of other acute diseases." " But these frequent lesions of the mucous membrane of the alimentary canal and of a variety of other organs, in patients who died of acute diseases of any nature, prove that when an affection of this nature gives rise to a febrile excitement of any duration, the majority of the organs of the body become the seats of more or less serious lesions.' "—*(M. Louis, Vol.* 1, *pp.* 381, 382.)

Now follows immediately the extract contained in my Essay :—

"6. 'The *mucous membrane* of the alimentary canal [in the typhoid affection] is not oftener, in fact is less frequently, diseased than some others, the spleen, for example, which was more or less seriously changed in all the cases of the typhoid affection excepting *four*. This is an IMPORTANT LAW, and it may tend, as it appears to me, to *simplify much the* STUDY OF PATHOLOGY. This is what we ought, perhaps, to have discovered A PRIORI '!—(See p. 682, *Sprengell*, and what our author says of "*a priori*" reasoning, p. 680—681.) "

Upon this extract I remarked,—" This is also a striking exemplification of our author's disregard of the symptoms of disease, and that the whole of his pathology consists in lesions of structure ; since, in all the fatal cases of the typhoid affection, but 3, there occurred diarrhœa and pains in the abdomen, and they were generally the earliest symptoms." —*(Com.*, p. 700.)

The reader must now understand, since something is said about *fever* in the foregoing extract, that my author refers this, also, entirely to the structural lesion of Peyer's glands, in the typhoid affection, as he does to the *liver* in yellow fever. Thus, from my Essay :—

" The following generalizations show that our author ascribed the whole febrile action to the structural lesion of the glands of Peyer. Thus :—

" 135. ' We must allow that the *febrile excitement* which was observed in the typhoid affection was *as much proportioned to the state of the small intestine*, as that which occurs in *erysipelas* of the face is to the *extent* of the skin inflamed.'

" 136. ' The FUNDAMENTAL ALTERATION, that of the elliptical patches of the small intestine, was more extensive according as the patients died more rapidly, and *the febrile action was generally in proportion to it.*'

" ' *The intensity* of the *febrile* action *is itself in proportion* to the extent and *seriousness* of the SPECIFIC lesion of the small intestine.'

" 137. ' Between the SYMPTOMS and LESIONS, of which we are now treating [glands of Peyer], the relation seems to me to be *not less evident* than that which is observed between *those two orders of facts*, as they take place in other affections; *pneumonia*, for example,' &c.

"'But, must we suppose this action of the *lungs* and of *the elliptical patches* of the small intestine to be the effect merely of a *sympathy*, which we cannot appreciate, *in their capabilities of producing disease, or as the consequence* of a *febrile* excitement of which the *inflammation* of these organs was the *source*. This last supposition seems to me to be the most probable.'

"'Their great importance [the facts previously given], as it seems to me, consists in this, that they ought to excite doubts in our minds, *as already stated*, in relation to *all febrile affections without a determinate seat*, and which are called *general*, and thus put us in the way of proposing problems which it is of great importance for us to solve.'" Again:—"'The number of these febrile affections without any determinate seat or *local lesion* is every day becoming more limited.'"

"And then follows, in a note, a reference to one of those '*isolated facts*,' which it is our purpose to notice soon; and about which our author hesitates whether he 'shall *prejudge* by it the important question of the nature of fevers.'"

"The fever had its origin, and continuance, in the alteration of the glands of Peyer, even when that alteration was '*latent*.' This refers to a particular case. Thus:—

"'Perhaps the reader will ask, if these febrile symptoms, without *diarrhœa*, or *pains* in the abdomen, observed during the first ten days [!], were connected with the commencing *alterations* in the *elliptical patches* of the ileum. The *affirmative* to this question will not appear doubtful, when we remember that the most severe lesions were those of the small intestine.'"

"138. 'If, then, FROM THE COMMENCEMENT of the affection *until its fatal termination*, ALL the morbid phenomena *were dependent upon*, or connected with THIS SAME lesion, it results that this *can remain* LATENT, during a considerable length of time, or at least *give rise to no characteristic* symptom.'

"Nearly a page is devoted to a minute detail of symptoms which took place antecedently to the arrival of the patient at La Charité, upon which the whole conclusion turns, and an account of which was obtained from the 'ivory-turner' (the patient) himself."—(Pp. 756, 758.) I may also add, that the patient died on the 26th day of disease, and that "*he said* he had been ill during *three weeks*" of that time. This case offers a fair example of my author's philosophy, ratiocination, pathology, origin of his knowledge as to the primary seat of the typhoid affection, &c.

As a question in relation to the *tongue* will come up, I will add here an extract which bears directly upon it, since it presents, also, a summary of my author's views of the controling agency of Peyer's glands. Thus:—

"59. 'Therefore, all the different conditions of the *tongue* observed during the course of the typhoid fever must be considered as the result of *one and the same cause* which is *common* to them with *all secondary lesions* which come on, like the former, at *various* periods of the disease ;' —that is to say, the glands of Peyer."

I must break off abruptly for this week.

To the Editor of the Boston Medical and Surgical Journal.

Sir,—I will thank you to omit the publication of the four remaining numbers of my Reply to "H. I. B." During the progress of the preceding, I have apprehended that I might be encroaching too much upon your Journal and upon the rights of other correspondents. This consideration has led me hitherto to request the delay of my Reply, should it interfere with other communications. I perceive by your notices that such communications are accumulating, and I am therefore entirely satisfied that the subject should now rest upon my part, so far as it respects the Journal.

There is one remaining point, however, which requires explanation; all other charges being groundless. I am accused of a misstatement in affirming that "Louis says that the indications to be drawn from the state of the tongue are the 'least important' of any. But we beg the reader to mark well, Louis never stated this, that we can find, in his work on typhus, or in fact in any of his works, so far as we can discover."—(P. 75.)

The words "least important" do not bear the marks of quotation in my work, and it is an induction of my own from extensive premises. It occurs in the following connection and rather incidentally, in Vol. 1, p. 238. Thus.—"They (Galen, &c.) speak of the important indications of the tongue. Baglivi says it supplies the most important, M. Louis the least." In a note I have referred to M. Louis "on Typhoid Fever *passim*." The statement, however, is notoriously true ; and the doctrine was the subject of no little comment by reviewers, a few years ago. In my Essay I have presented several extracts from my author to the foregoing effect. The generalizations numbered 57, 58 and 59 are entirely of that import. See, also, M. Louis on Typhoid Fever, Vol. 2, p. 56.

Dr. Bowditch recurs to the foregoing subject at the close of his attack, and imputes to me "a false statement," which he allows to have grown out of an error in his own translation. In consequence of this error on the part of one who had declared his belief that he had translated his author correctly "in every respect," I was betrayed into a partial misapprehension ; and this is the only instance in which any departure from strict exactness can be truly alleged against my Essay.

"What," says Dr. B., "can we think of the candor of such a writer, when he takes the first line of a sentence and makes pages of commentaries upon an isolated assertion," &c. "Our commentator may complain that the sentence was *badly constructed*. We affirm that this is no excuse; for, in the *original*, the *meaning* is perfectly plain, if the punctuation in the translation is faulty."—(P. 107. *My Italics.)*

Dr. Bowditch has given a pretended quotation of six lines, which embraces "the first line" in question; but it is made up of words selected out of more than a page and a half of M. Louis's work, presented as a continuous sentence, and bears the usual marks of a quotation. I shall therefore produce the statement as it occurs in the original translation, observing, also, the punctuation. Thus:—

"1st. *Tongue.* It was almost always natural; that is, it had not any unnatural redness; it was moist and was at times only a little yellowish and whitish in nineteen patients; among whom were all those who died between the eighth and fifteenth days of the disease, and who arrived at the hospital at a sufficiently early period to be examined with care upon this point."—(Vol. 2, p. 55.)

The words quoted, and rather incidentally by me, are "*The tongue was almost always natural;*" to which I add, that this is "contradicted by nearly every one of his exemplifying cases." Now, as the extract stands, the first clause appears as a direct and unqualified affirmation, whilst the details which follow purport a contradiction. These contradictions met me so constantly, that I was here led into an *unimportant* error by the faults in the translation, and, if it can avail its author anything, I take more satisfaction in allowing it, than I have done in the preceding exposition. I had imagined, indeed, from the structure of the sentence, that instead of a semicolon after the word *redness*, there should have been a period. Such, I submit, is the only rational mode of interpreting the paragraph.

But, the error, I repeat, is unimportant, and the clause may be expunged from my Essay, without affecting, in the least, the merits of the question. The fundamental point is set forth in other quotations from my author by which the foregoing is preceded; whilst the affirmation, as quoted by Dr. B. that "the tongue was natural or nearly so in a little less than half the cases," also bears out my commentary that this is "contradicted by nearly every one of his exemplifying cases." This will be seen at once by a reference to my tables, where I underwent the labor of condensing, in their "consecutive order," all the appearances of the tongue in each of the fatal cases. But this was not done with a view to the specific fact in question, but to the general affirmations of my author that little or nothing could be gathered from the appearances of that organ, especially in relation to the stomach. There is not one of the cases, unless it be No. 32 (in which the tongue was "greyish in centre"), that can be regarded as offering in any sense a "natural" condition. My comment upon the statement in question, instead of consisting of "pages," extends only through about a dozen lines.

That a fair opposition would be made to my Essay upon the writings of M. Louis, as well as to many doctrines taught in my work, I was entirely prepared to expect; but, the forbearance with which they have been treated by the medical press of this country, and the commendations of the work which have wholly surpassed my expectations, render it proper that I should embrace this occasion to express my profound gratitude for so much generosity.

In conclusion, I may suggest the consideration, that what may have borne the aspect of personal severity in the course of my Reply, has been the unavoidable result of my premises, and of the necessary demonstration. The attack was peculiarly personal, and consisted throughout of a series of misrepresentations. It was made by a responsible individual, and through the columns of a distinguished Journal. It related not only to a laborious work which was designed for the public, but to my reputation as a man. I have felt, therefore, that I was at least entitled to a hearing in my own behalf, if no interest existed in relation to my public labors. I have also prepared for the press other works which it is my intention to publish, and it was therefore the more necessary for their present success that I should avert the possible consequences of those misrepresentations which were designed to affect me unjustly as an author. Nor can I permit myself to doubt that every honorable mind, when it shall have regarded my case as its own, will arrive at just conclusions.

The foregoing Reply will be republished, with the remaining refutation, for the purpose of binding it up with my "Commentaries," that its truth may be tested by the work itself, and effectually protect the honor of its author. M. P.

Dec. 9, 1840.

THE remaining assumptions of Dr. B. involved the necessity of many quotations from my Essay, which I had embraced in the residue of my Reply, and which formed another motive for omitting their publication in the Journal. Charges may be more briefly made, especially where truth is disregarded, than their refutation; and, in this respect the accuser has always an advantage. This I had constantly felt; and now, to avoid any prolixity of detail which may be repulsive to some readers, I have substituted references to my Essay for the extended quotations.

Returning to the question concerning diarrhœa, which was a subject of examination in my fifth number, it will be seen by referring to my Essay, page 759, that I went farther than I was warranted in underrating my author's real belief that diarrhœa depended upon a lesion of structure, and certainly beyond anything that is justified by my quotations as to the dependence of diarrhœa upon a structural lesion in the typhoid affection.

It is obvious from the foregoing reference, that I yielded an advantage to my author's theory of the *primary* seat of the typhoid affection, to which he was not entitled. The reader will have seen the difficulty with my author. The *anatomical* lesion of Peyer's glands was assumed to be the very first in the series of morbid changes, and it was necessary, therefore, to assume, also, that the first symptoms depended upon it. Diarrhœa was almost always one of the first. Hence the object of my argument in the foregoing reference to my Essay; for, whilst I was willing to allow what my author might not be inclined to take, on account of the *glandular* doctrine, I could make out my case that the affection of the *mucous* tissue, *per se*, being primary, the glandular lesion might be only a *secondary* result, and thus sap at the foundation the general doctrine which ascribes all the consecutive lesions to the assumed primary lesion of Peyer's glands.

Again, to show how fully I have represented my author upon this, as on all other questions, I have even presented him as denying a morbid state of the intestinal mucous membrane in *Enteritis*. Thus:—

"141. 'We are led to believe that the mucous membrane of the intestine is but little altered in *Enteritis*, but that it is in a condition which differs perhaps but little from that of the *skin* when it is the seat of copious *perspiration*."—(P. 764.)

Here, as every where else, we have a practical illustration of my author's conviction of the "paramount importance" of morbid anatomy. I introduced at the close of my fifth number several quotations showing, perhaps with sufficient comprehensiveness, my author's views as to the dependence of the whole array of the typhoid phenomena upon the lesion of Peyer's glands. In my Essay, I went into an elaborate consideration of the subject, and presented a multitude of extracts, gathered from all parts of my author's work, to the foregoing effect; partly because it had been affirmed by my author's friends that "M. Louis did not show nor did he attempt to show, that the disease he described was dependent on the morbid affection of the small intestines," and partly to carry out the intention of the Essay of supplying a practical demonstration of the evils of morbid anatomy when made the great basis of pathology. What, for instance, should be the remedies for *enteritis*, if my author's doctrine be sound, as propounded in the foregoing quotation?

In addition to the extracts in my fifth number, I would now refer to the generalizations marked 120, 123, 124, 126, 128, 129, 130, 131, 132, 133, 134, 142, and many accompanying quotations of the same import, as direct affirmations of the dependence of the phenomena, and the various lesions of organization, upon the structural lesion of Peyer's glands, in the typhoid affection, and, as in most other citations, fully indicative of my author's almost entire dependence upon morbid anatomy for his pathological inductions; about which all correct treatment of disease is necessarily concerned. The whole fabric, in the typhoid affection, turns upon the lesion of Peyer's glands, whilst the subjects themselves were of the worst class, the disease far advanced before their admission (see my tables), the primary symptoms gathered from the purlieus of Paris, the pathological specifications depending mainly upon *shades of color*, and the dissections made on an average more than 29 hours after death—often 40, or more. (See tabular view, p. 799.)

The practice of referring the principal attributes of disease or the ulterior results to some particular part which may be found by one anatomist or another most frequently deranged in its structure, has been a prevailing error at former periods, and many philosophers have attempted its exposure. I have quoted from Sprengell, Zimmermann, and others (p. 682, &c.), to show the coincidence, in this respect, of my author's philosophy, not only in regard to the typhoid affection, but the yellow fever, and other diseases. The reader will now comprehend the imputed value of the "900 cases," as examined in my second number, and may decide whether, "in fairness" to science, it was not rather my duty to have placed the whole merits of my author's work upon the morbid anatomy of "the 50 cases of the typhoid affection," than to have admitted other elements which, in reality, formed no part of the ground for the pathological inductions.

And now, in what language shall I speak of Dr. B.'s denial that M. Louis's generalization of the anatomical lesion of Peyer's glands was designed for all climates of the globe, and of his imputation of untruth to me for this intimation? Let the reader turn to the preceding quotations and references, which not only make that lesion indispensable to the typhoid affection, but to all other lesions of structure, and to the symptoms in a general sense. And let him consult my Essay from page 730 to 739 as to the generalizations in respect to remedies, by which my author abolishes bloodletting, blisters, &c., not only in the typhoid affection, but in the gravest internal inflammations,—and from page 700 to 705 as to organic lesions; and he will here find, as throughout my Essay, that my author's conclusions were designed for "laws" of universal application. That my author has avowed a disposition to admit the results of farther observation, that he has acknowledged a diffidence of opinion, and commended a pursuit of "the numerical method," I have fully represented in his own language, and with such comments as seemed to me appropriate. But, all that is thus incidentally said in behalf of farther inquiry affects in no respect the unreserved manner in which he lays down fundamental laws and generalizations, and in opposition to his avowed object of supplying only a record of facts, and in violation of that principle upon which he rejects the learning, the labors, the philosophy, of ages. The allowances which are thus introduced, and in a general manner, only serve to impart greater force to the specific conclusions, whilst we are early admonished to beware of others:—"The reader will pardon us, perhaps, for having insisted so much on the care we have bestowed on the collection of *our* facts, and upon the *distrust* with which part of those *daily* published ought to be received," &c. (On Phthisis, Preface, p. 67.) All the pathological inductions in respect to the great disorganizations in the typhoid affection are absolutely expressed in a fundamental sense, and without reservation.

Again "Dr. Paine states," says Dr. Bowditch, "that the follicular affection is denied to exist by Louis and his followers in genuine typhus. W know not to whom Dr. Paine refers under the title of followers;

but, as it regards Louis, we deny that he ever made such an assertion, and challenge Dr. P. to produce any proof to that effect."—(P. 90.)

The reader will have seen the "proof" in many of the citations and references already made. Not only "the follicular lesion," but as the reader will find, by consulting my Essay, several other "specific" lesions, are appropriated by M. Louis exclusively to "the typhoid affection." But, what are my words, that a charge is thus protruded as if some great injustice had been contrived,—perhaps some quotation mutilated, or some translation condemned, or some fraudulent substitute stealthily advanced? No such thing,—no such "trickery" as our translator has it. My words are incidentally connected with a statement going to invalidate the great attribute of the "typhoid affection"; and that statement was predicated by another writer of the very principle which Dr. B. denies with an assurance which is only equalled by himself. My words are:—

"Dr. Perry states the following as the result of 300 anatomical inspections in the genuine typhus, in which the follicular affection is denied to exist by M. Louis and his followers."—(P. 689.)

It is even impossible for the reader to know whether the imputed affirmation is made by Dr. Perry or by myself. But it was I that made it, as indispensable to the subject matter of proof which I was about to bring from various observers to show that the "follicular lesion" was not "peculiar to the typhoid affection." At the very page from which Dr. B. was elaborating this charge, begin the following remarks, which shew him the true position of my author as to "the typhoid affection" and "the genuine typhus." Thus:—

"In our country," says the British and Foreign Medical Review, "we do not require any arguments to persuade us of the identity of these supposed distinct diseases. The evidence is constantly before our eyes." "We think that the condition of Peyer's and Brunner's glands can, as little as the previous circumstances of contagiousness and period of life, be referred to as any ground for the distinction of typhus and typhoid fever." And thus Roupell:—"The impression on my mind, arising from the correspondence between the leading features as already detailed is, that the two fevers are identical."

Notwithstanding the notoriety of the fact, and the proof which was before the eyes of my critic, when he wrote the foregoing remark, that a vast series of observations have been made in Britain to show the existence of the follicular lesion in "the genuine typhus," and to thus disprove the assumption of M. Louis that it is an anatomical lesion "peculiar" to the typhoid affection, and, although much of M. Louis's real fame rests upon this assumption, let us look at only one or two of the numerous quotations of a concurring import, for the specific "proof" which is demanded. Thus M. Louis:—"We must not consider it merely as peculiar to typhoid affections, but as forming their anatomical characteristic *as much as tubercles do that of phthisis*, whatever may be the cause of their development;" [as much so at Pekin as at Paris].

Now it is notorious that M. Louis did not include the genuine or "contagious" typhus amongst the "typhous affections" of which he was speaking, and to the former of which Perry, myself, and others were referring. By "typhous affections," M. Louis means affections strictly analogous to "the typhoid disease." The plural also is of very rare occurrence in his work. This is the universal understanding. Take, again, another example,—the able and distinguished paper by Dr. Gerhard, in which he draws an absolute distinction between "typhus fever" and the "typhoid fever." "The typhoid fever," he says, "was placed by this work of Dr. Louis, in the same relation to other fevers that pneumonia holds in reference to the affections of the chest."* The name *dothinenteritis* was bestowed upon it, to distinguish it from all other fevers, just as *ornithorynchus paradoxicus* was invented to distinguish that animal from all other animals. Indeed, such was the assemblage of imputed aberrations in the "typhoid affection," that my author was embarrassed for a name, and declares in his "advertisement," that—"For a long while he endeavored to find some word, which would express the anatomical characteristics of this disease, without being disagreeable to the ear;" and he actually abandoned it in the end, as a hopeless enterprise. But, we have now got the euphonic word. If we therefore associate all this labor at a distinguishing name, and its original fruitlessness, with all the mysteriousness in which the various structural lesions are involved; that the inflammation and ulceration of Peyer's glands are declared to be of a "specific" nature and "peculiar to the typhoid affection," and that the structural lesions of the alimentary mucous membrane, of the heart, liver, spleen, epiglottis, pharynx, œsophagus, &c., are attributed to a cause "exactly the reverse of inflammation," and are superadded, as well as the rose-colored lenticular spots, the sudamina, the meteorism, the hard patches, perforations of the intestine, as "peculiar" also to the "typhoid affection," amongst acute diseases, we have a grouping of attributes which present that disease as a perfect anomaly in human maladies, and as even falling without the pale of any known laws in the animal economy. (See Comm. pp. 701—709, 712, 719—726.) It is therefore *sui generis*, a perfect *Morbus Paradoxicus;* and were not the name already appropriated to another celebrated complaint, I would call it the *Morbus Gallicus*."

But to put this matter beyond all question, here is my first of numerous quotations to the foregoing effect, viz.:—

"120. 'I have considered it [the alteration of the elliptical patches] as *inseparable* from the disease we are now studying, and as absolutely forming its anatomical characteristic.'" The French—"je l'ai regardé comme inséparable de l'existence de l'affection qui nous occupe," is very expressive. This, therefore, *per se*, is equivalent to denying its existence, not only in the "genuine" or "contagious" typhus, but in all other acute diseases.

We will now have a practical illustration, in which M. Louis enforces the peculiarity of the lesion to "the typhoid affection," "qui nous occupe," by comparing it with another disease, as we have just seen of the individuality which he establishes for the typhoid affection, by connecting it with the follicular lesion as he does tubercle with phthisis. Thus, of Obs. 41 :—

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* American Journal of Medical and Physical Sciences, Feb. 1837, p. 290.

"Save in respect to some modifications I shall soon mention, the lesion of the small intestine did not differ in the least from what is observed in subjects who die of the typhoid affection, after having experienced the gravest symptoms. We cannot, therefore, fail of recognizing the character of the disease in this case. This subject had the typhoid affection as much as he has had pneumonia, in whom we find hepatization of the lungs. One case is not less certain than the other" (p. 714); and as good for one climate as for another. And thus again :—

"35 and 36. 'Ulcerations of the œsophagus, like those of the pharynx, seem *peculiar* to patients dying of the affection which we are now examining."

"37. 'Therefore, we must consider ulceration, or partial destruction of the epiglottis, as one of the secondary anatomical characteristics of the typhoid affection, as we have already decided that ulcerations of the pharynx and œsophagus are; and that this lesion observed in a patient who should die of any acute affection proves almost to a certainty, *without need of further examination*, that the patient had died of *typhoid fever*.'"

"21. 'Although the hard patches are found in a fourteenth part only of the cases of typhoid fever' [less than four cases], 'it is nevertheless evident, that they are of great value, since, as they appear to be *peculiar* to typhoid fever, they would be sufficient [universally] to enable us to recognize the affection at the first glance.'"

"22 and 23. 'The meteorism and ulcerations of the large intestine are not less important than the hard patches.'"

"29. 'In those cases in which the meteorism is very much marked, we may be able, at the first glance [universally] to *distinguish the Corpse* of a person who had died of *this* disease [the typhoid affection] from that of one who had died of any other acute disease'"!

"26. 'Although in the actual state of science [!] I cannot assign any cause for meteorism, I would, nevertheless, remark that it seemed to me to follow a LAW which was analogous to that by which other symptoms are governed." "There is something *specific* in meteorism'"!

"11. 'The morbid change of the *spleen* has something *special* and *characteristic* about it, in patients who are attacked with the typhoid affection.'"

"30. 'Perforation [of the intestine] in the course of acute diseases, is *peculiar* to the typhoid affection.'"

"31. 'The typhoid disease seems to establish a *marked* predisposition to morbid changes in the mesenteric glands.'"

"32. 'In certain cases we can *determine the period* at which death took place, by means of the mesenteric glands.'"

"40. 'Rose-colored lenticular spots seem to have in this disease something of a *specific* character,' and 'are something *peculiar* to the typhoid affection.'"

"41. 'Since sudamina appeared as frequently in the cases of slight attack as in those which were more severe, must we conclude that this eruption, like the rose-colored lenticular spots, is something *peculiar* to the typhoid affection ?' "

The reader will find in my Essay, various accompanying remarks, and other quotations of a like import, which are too long for repetition, and are clearly unnecessary to decide the question at issue. But, Dr. B. has implied by the foregoing denial, that M. Louis had not restricted the follicular lesion to the typhoid affection, and impugns my veracity in having made such an affirmation, although the foregoing extracts, and others like them, were at the same moment before him. I will therefore convict him out of his own mouth, whilst I show to be true, at the same time, what he is disposed to deny of M. Louis's followers. In his attack (p. 82), he quotes Chomel's opinion (but with whose *facts* I was concerned), to show that I had not correctly represented his (Chomel's) statement as to the anatomical lesion of Peyer's glands. Thus :—

" We conclude from these researches, depending upon numerous observations, agreeing in the most important particulars with those made by Mons. Louis in Paris, and Dr. Bright in England, that the alteration of the intestinal follicles is a condition *wholly peculiar* (tout à fait particulier) to the typhoid affection, the different periods of which we can follow as we can *those of an abscess or a cutaneous exanthema.*—(Leçons, p. 222.)" *The Italics belong to the Critic.*

The reader will have observed that the foregoing quotations are also applicable to other parts of the attack.

I wish not to involve unnecessarily any others in this Reply; but, perhaps I may properly introduce the frank admission of one of my New York Reviewers, and M. Louis's defender, that,—" we fully join with Dr. Paine in thinking that the writings of this author (M. Louis) fully justify the conclusion that he does regard the affection of Peyer's glands *as the disease*, referring the delirium and other remote symptoms *directly* to it."*

Dr. Bowditch "hastens to another instance of our commentator's unfairness," and "heartily detests the trickery."—(Pp. 92, 93.) The subject of complaint is the following passage from my work :—

" Our author has no difficulty with *analogy* where a lesion of structure may embellish the philosophy of disease. Thus :—' Analogy,' he says, ' is in favor of what we advance. For, when hemorrhage occurs in any internal organ, it is almost constantly a symptom of more or less alteration of structure.' From this assumption he reasons analogically that—' hœmoptysis, with certain exceptions, whenever it occurs, renders tubercles in the lungs *infinitely* probable.' " I then add,—" Here, as we shall see in many other instances, our author makes a demonstrative, though a bad use of analogy, since his *premises being just otherwise*, analogy operates against him."—(P. 696.)

Now the " trickery " consists in suppressing my commentary, which discloses my object, and represents the whole just as it would have been had I encumbered my pages with the long extract which is prefatory to my quotation, and which is given by Dr. B. to show what I had stated, —viz. that " *his premises* were just otherwise." Here, too, as in the instance regarding the dependence of diarrhœa upon structural lesion, I was employed exclusively upon the subject of *analogy*, to show, by the habits of my author, how unjust he had been to all other writers, and to science. But, this object is studiously concealed by Dr. B. in both cases, as is also the fact that I had in view one of my author's numerous contradictory statements, and by the concealment of which, and by perverting the import of my subject, Dr. B. represents me as guilty of the very "trickery" about which he was at that moment employed himself. It is exactly of the nature of that which I examined in my second number. So, also, in quoting my extracts from M. Louis upon *one side*, Dr. B. would lead the reader to believe that I had not presented the other,—when, in fact, it was one object of my Essay to exhibit my author's incongruities.

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* New York Journal of Medicine and Surgery, October, 1840, p. 428.

Again, as Dr. B. says,—"We have another specimen of our commentator's unfairness, with either a disposition to lead the reader astray, or great carelessness in quotations."—(P. 94.) This consists, as Dr. B. affirms, in my having brought two passages from M. Louis's work in juxta-position, without a proper amplification, to show an inconsistency. The statement, however, which is brought by Dr. B. from a very different part of M. Louis's work, only makes the "inconsistency" more apparent. The subject of complaint relates to my author's doctrine, as quoted in my Essay, that, "it is nearly correct to state that the apparent condition of the brain cannot explain the symptoms of which it is the source, any more than the mucous membrane of the stomach can account for the anorexia and other gastric symptoms in the great majority of cases."—(*Gen.* 142.) Dr. B. does not like the unqualified manner in which my author lays down this doctrine. That it is exactly so, however, the reader will ascertain by referring to other quotations in which the same doctrine is expressed in the most unqualified manner, and which are marked in my Essay as generalizations 120, 121, 122, 123, 124, 125, 126, 127, 128, 129, 130, with other copious extracts, of a general and specific nature (especially the anatomical at p. 763, &c.). The fact is too obvious for comment, and, as has been stated, is allowed by a defender of my author. Nor could the least qualification of the doctrine arise from what Dr. B. would introduce as to the mucous membrane of the stomach; since his quotation from a remote part of the work shows a violence of gastric symptoms in *two thirds* of the cases, and a "serious alteration of the mucous membrane of the stomach," in one *third*. And yet in the very face of this quotation Dr. B. adds, "in other words, a proportion of *one half* the cases, but *no corresponding* lesion." It is also more than once said by my author, that "the mucous membrane of the stomach was, in the *greater proportion* of cases, more or less seriously altered, sometimes softened and thinned, or even destroyed."—(P. 766.) It is also worthy of remark that my author, when speaking of the structural lesions of the stomach, in the foregoing specification, was employed about the paramount importance of morbid anatomy; so that, it will be seen that Dr. B. has not improved the consistency by introducing an extract which renders my author regardless of the condition of the stomach, and sacrificing every other alteration of structure to establish the universal dependence of the symptoms upon the lesion of Peyer's glands.

Dr. B. had been quoting from my Essay at page 766 (Gen. 142), where the extracts are separated by comments upon each. By turning to that page, the reader will see that I had a higher object in view, in this instance, than of making out an "inconsistency," which is put forth by Dr. B. as the only one. He will next observe that I have here employed no such expression as imputed to me,—"that Louis thinks that the symptoms of disease of the stomach are very obscure," whilst our translator, in quoting my foregoing extract, suppresses the words, "sometimes softened and thinned, or even destroyed."

The most culpable act consists in imposing the belief that the whole of this subject had not undergone a full investigation at a preceding part of my Essay, and that I had not represented all my author's antagonizing views for the purpose of proving an "inconsistency." This, as I have said, is one purpose of my Essay, that I might the more effectually invalidate my author's generalizations, and, also, by thus presenting all the conflicting doctrines, leave no opportunity of imputing to me an "unfairness,"—which is the whole burthen of Dr. B.'s attack. That this was fully done in relation to the stomach, the reader will find by turning to pages 700—701 (*Gen.* 4, 5, 6, 7, 8), 744 (*Gen.* 118, 119). Also, p. 431—433. The school of Broussais, it will be here seen, is in a measure answerable for my author's doctrines. This appears especially in the second paragraph of *Gen.* 7; and, to render that more intelligible, take the following which occurs in my author's work in connection with what I have there quoted. After the words "most obscure," my author goes on,—

"We can, however, understand why science has made *so little progress* on this point, when we perceive that simple gastritis, or at least what is originally simple gastritis, and which causes death, is of very rare occurrence." "It is impossible, therefore, under these circumstances, *to recognize any symptoms* except those of the *most serious lesions*, whilst those dependent upon slighter ones, inevitably escape our notice; but, nevertheless, it is *impossible to know the value of symptoms before we compare them with the condition of the organs*."—(*M. Louis, Vol.* 2, *p.* 44.)

The foregoing extract should, also, be considered in connection with what has been said on the subject of "diarrhœa," and "the paramount importance of morbid anatomy."* As to the complaint of "bringing together," how is an author to be appreciated upon questions of the foregoing nature, without a comparison of statements?

Dr. B. objects (p. 95) that I did not quote M. Louis sufficiently as to his induction, that softening of the heart was owing to a *cause* which is thus expressed by my author:—

"14. 'As I remarked in relation to the softening of the liver, if we knew any cause of disease *exactly the reverse* of inflammation, it would be proper to refer this softening to it."

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* See my Essay on the Schools, Vol. 2, p. 659.

Now the whole of this conclusion is predicated of mere considerations about color, and the absence of "pus from the walls of the heart." But, Dr. B. thinks that I did not quote my author sufficiently about the shades of color, in this instance,—and this is the gist of the complaint. Nevertheless, I did state every word that is in the least essential to a full understanding of my author, even as it respects the color. But, although color is the main foundation of my author's pathology, and of his generalizations of the structural lesions in the typhoid affection, I might, with entire propriety, have neglected all allusion to it, as it was my object in stating that generalization, to show that my author assigned the *softening* of the *heart* in the typhoid affection, like that of the spleen and liver, and the ulcerations of the intestines, to "a cause exactly the reverse of inflammation,"—leaving it to the reader to make the best of the pathology, and its appropriate treatment. I felt inclined, however, to let my author have the benefit of his philosophy as to *color*. But, Dr. B. has wholly perverted the plain object I had in view, which was to state an important generalization in pathology, founded upon morbid anatomy; whilst Dr. B. represents it as purporting, especially, to assign the *reasons* for a generalization. Had not Dr. B. also suppressed a part of my extract, and had he said (what he also studiously conceals in other instances), that it was merely offered as the expression of a generalization, the reader would have seen at once what I shall now enable him to understand by presenting the proper extract, which embraces the substantial premises of my author. The words in Italics are those which are suppressed by Dr. B. Thus:—

"14. '*The heart had less consistence than natural in 24 subjects.*' 'It was generally livid and purplish on its surface as in its substance. The internal face of the ventricles and auricles was, on the contrary, of a deep-violet-red color.' '*As I remarked in relation to the softening of the liver, if we knew any cause of disease exactly the reverse of inflammation, it would be proper to refer this softening to it.*'"—P. 702.

Now, it is true that Dr. B. has the words which occur after the word *liver*, but disconnected from the extract, and so managed as to purport their absence from my quotation, and even as being extracted by himself from M. Louis, whilst he makes me tacitly refer to them.—(P. 95.) The omission of which Dr. B. complains, on my part, are the words, "It was of an onion-peel color," which precede immediately the foregoing statement beginning with, "it was generally livid," &c. Thus:—"It was of an onion-peel color, and was generally livid and purplish," &c. But, it will be seen that the "onion-peel color" is only a less definite statement than the subsequent, which, being specific and coincident with the former, rendered that superfluous. Dr. B. also complains that I did not add after the words "of a deep violet color," the words—"which color sometimes penetrated beyond the lining membrane, and appeared owing to an *imbibition* of blood, which it resembled more or less in color." My critic farther complains that "where Louis says 'that if we knew any cause of disease,' &c. the commentator says 'our author *refers* to the absence of pus in the walls of the heart, as a special proof of the foregoing doctrine'; whereas Louis uses this fact, and likewise the non-existence of pericarditis in any of the cases [being my critic's inference from the 'sometimes imbibition of blood'], as merely considerations to support, in some measure, his previous arguments [the matter of color], and which to any fair mind are sufficient." "Dr. P. takes what Louis uses as merely supplementary to the main argument [color], and puts it forth as the chief ground-work."

The reader, by comparing my quotation and remarks with the foregoing, will readily perceive the manner in which they have been perverted. I may say, however, that here, as every where else, I have fully represented, in the language of my author, the hues of color as constituting, what appears now to be allowed by Dr. B., " the chief ground-work " of my author's pathology. My commentary adverts to *pus* as only "supplementary" proof, or " a *special* proof;" *color* being the *fundamental* one.

In that part of my article which remains unpublished in the Medical Journal, I went into a long examination of Dr. B.'s statement that "Louis never did propose to decide that a part was inflamed from the existence of redness merely."—P. 95—98.) This I had mainly done to illustrate a doctrine which extends beyond my author, but which is more amply considered in my work. The long extract which Dr. B. has made from my Essay places the foregoing question in its true light, and confirms my author's dependence upon color as a principal element in his pathological considerations, or as Dr. B. has it, in contradiction of himself, forms his "chief ground-work." This is remarkably apparent on almost every page of my author's treatise on the Typhoid Affection, and is variously exemplified in my quotations. " Ulceration," " thickening," " intestinal perforation," " coagulable lymph," " softening," vast " peritoneal adhesions," " glueing together of the intestines," &c. many of which occurred in nearly every case, are sacrificed, as insignificant of inflammation, to the paramount importance of *color*. Illustrating cases of all the foregoing occur in my work. (See Vol. 2, pp. 631—633, 748, 798, 712, 719—721, 749, 319—330.) In the last reference, as also in the very citation which is made by Dr. B. from my Essay, I have quoted my author as having once considered *color* (in his work on Phthisis), as of inferior moment to the greater physical results, but as having abandoned the doctrine in his work on the Typhoid Affection. (See pp. 320, 703.) The school of Broussais had now opened its vast jaws to approaching dislocation, and threatened to swallow up not only my author, but more real giants. My author, too, had a great enterprise to carry, in the glands of Peyer, and to have allowed of inflammation in any other part, especially of *gastritis*, would have been a weapon for his adversary. I considered my author's revolutionary doctrine of immense moment, especially as being sustained by M. Andral and other powerful philosophers, and I endeavored to discharge my task with equal justice to mankind and to my author, and with better fairness than the latter has disposed of his brethren "from Hippocrates to the dawn of his own empire."

Numerous citations occur in my Essay of a direct nature, which establish my author's views upon the question before us. I shall now introduce an example of the whole. Thus :—

" 10. 'What deduction shall we make from these facts? Must we admit that the *white* and *red* softening have each their own causes; the one *wholly different from the other!* This question seems to me may *now be decided affirmatively*, at least in certain cases.' "—(P. 701.)

Take, also, the quotation which occurs exactly at the place where my critic broke off his extract from my Essay. Thus:

" 'There is no doubt,' he says, 'that in the first two cases the softening was inflammatory, and that such also was the fact in the third and fourth, *in those spots* where there was a *red* color. But, was this the fact for *those spots* where the mucous membrane was *pale*; and the fifth case, in which throughout the whole extent of the canal there was merely a *gray* color'?" &c.—(P. 704.) The investigation is then carried on under all its aspects, till, remotely from the foregoing, at p. 751, a general summary is presented.

As to the "certain cases," in the foregoing generalization, the reservation is beneath the dignity of science, and is contradicted by the whole of my author's pathology, and reasoning upon color, throughout the work on the typhoid affection. The reader, however, will see, by referring to Dr. B.'s long quotation from my Essay (*Com.* p. 702—704), that I did not affirm what is imputed to me by Dr. B., but that "the inductions rest *chiefly* upon the fact that the foregoing ALTERATIONS of STRUCTURE are *white* in one case and *red* in the other"; whilst perversions of the foregoing nature are interwoven with all parts of the attack, and are even arrayed against the plainest import of accompanying quotations from my work.

It is evident, however, from what Dr. B. has inadvertently admitted as the "ground-work" of my author's pathology, and from introducing his own testimony to sustain the foregoing affirmation, that his convictions are opposed to his professions; since, he represents M. Louis as having once declared in his hearing, "that color alone indicated nothing." Very possibly; but Dr. B. should have recollected that it is not *in the book;* and, moreover, that he had disqualified himself as a witness by affirming that, "we are unwilling to take the assertion of any man."—(P. 75.)

Dr. B. is disposed to think, or rather to imply, that I have not represented M. Louis fairly as to bloodletting in pneumonia. Nevertheless, I have quoted him extensively and fully upon the subject. At pages 730 to 737, there are more than *twenty* distinct extracts respecting this remedy in different acute affections, *eight* of which relate to its application in pneumonia. Other quotations respecting the remedy occur in other parts of the Essay, and under every aspect; but they are entirely too extensive for repetition. The reader, indeed, will be surprised to learn, that my particularity reaches to statements of the effects of bloodletting in pneumonia and the typhoid disease at various stages of the complaints. The remedy is condemned, on the whole, in that desolating inflammatory disease, "the typhoid affection," and is considered about useless in pneumonia, and other scarcely less formidable inflammations. As supplementary to what I have quoted in my Essay, I may add the following, which conveys a just view of my author's opinion of bloodletting in the three inflammatory affections which form the subjects of investigation in his treatise on that remedy, viz. :—

"From the exposition of facts in this chapter, we infer that bloodletting has had *very little influence* on the progress of pneumonitis, of erysipelas of the face, and of angina tonsillaris, in the cases under my observation ; that its influence has not been more evident in the cases bled *copiously and repeatedly*, than in those bled *only once and to a small amount ;* that we do not at once arrest inflammations, as is too often fondly imagined ; that, in cases where it appears to be otherwise, it is undoubtedly owing, either to an error in diagnosis, or to the fact that the bloodletting was practised at an advanced period of the disease, when it had nearly run its course."—(*On Bloodletting*, p. 22.)

I wish not to resort to any foreign help upon this question, as the proof is ample in my Essay, as derived from my author's works. Nevertheless, I will say, that his efforts to exclude bloodletting from the practice of medicine has been proverbially known ; and the Medico-Chirurgical Review, in referring to the subject, speaks of it in that manner, and as being commended by his followers. Thus :—

"It would seem, however, that he (M. Louis) has not abandoned the practice (bloodletting), although we were assured that this conclusion was drawn from the most accurate arithmetical and statistical data ! How are we to place *any confidence* in the practical directions of *such a writer*, however high his attainments as a morbid anatomist may be ? And yet, some of our most popular authors of the present day attempt to laud him as another *Sydenham* or *Morgagni !* "—(Medico-Chirurgical Review, Oct. 1839, p. 543.)*

Although Dr. B. is not satisfied with my "fairness," he complains of my prolixity. But this is owing to the profusion of my quotations from the works under review. Besides exhibiting my author's doctrines in their various aspects, I performed the unexampled task of placing a marginal reference to nearly every one of the quotations. Besides 32 extracts from various works by my author, expressive of his sentiments of the profession, there are, in this Essay, 470 distinct marginal references to his works. The text also embraces many other analytical notices with references which do not appear in the margin. Thus, at pages 732—733, there are 21, and at page 799 there are 54. There is also a tabular view of six pages, and of great labor, in which I have analyzed the whole of the 54 fatal cases, having affixed to each case the duration of the disease before admission, how long diarrhœa had existed, cathartics, enemas, bloodletting, leeches, other remedies, diet, appearances of the tongue in the order of occurrence, and how long the patient survived the treatment. I have also brought together the whole fabric of 151 great "laws" and generalizations in pathology, and therapeutics, duly arranged according to their affinities and repulsions, illustrated by the ratiocination of my author, with a copious detail of the qualifications of the subjects upon whom the observations were made; having thus, and in various other analogous respects, carried out the requisitions of the "numerical method." The friends of this school, therefore, and those of the anatomical, should be the last to complain.

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* In my Essay I have the following quotation from M. Louis on Bloodletting.

"Need I repeat, that an excellent mode of arresting diseases is to confound them, or at least to make no distinction in the periods at which such and such remedies were employed, *as I have pointed out* in the preceding chapter."

This has been turned off, in another quarter, "as an ironical hit at Bouillaud and others." There is no such reference intended. It is the doctrine of my author, as fully shown by the last clause of the quotation ; and, if the reader will turn to the "preceding chapter," he will there find it fully carried out ; nor had there been the most distant allusion made to Bouillaud, or to any other observer. In the same journal one of my quotations from M. Louis is repeated by the Reviewer, and an objection raised which will be found to depend entirely upon the commentator's neglect of an important fact, and of his omission of my marks of quotation, both in the extract and marginal references. The extract is the first of the series at page 693. It is an important one, and is obviously designed in this place as a simple reference to two principal doctrines of my author. But, at page 681—682, and again at page 706 (*Gen.* 17 and 18), the quotation is repeated in full, and according to its individual aspects. I notice the mistake from the convenience of the opportunity.

ESSAYS

ON THE

PHILOSOPHY OF VITALITY

AS CONTRADISTINGUISHED FROM

CHEMICAL AND MECHANICAL PHILOSOPHY,

AND ON THE

MODUS OPERANDI

OF

REMEDIAL AGENTS.

BY

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HISTORY SOCIETY, &C.

"The Lord God FORMED man of the dust of the ground ; and BREATHED into his nostrils the breath of life and man BECAME a LIVING soul." — GENESIS.

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1842.

THE FOLLOWING ESSAYS ARE RESPECTFULLY DEDICATED TO

PROF. J. MULLER, M.D.

A. P. W. PHILIP, M.D. F.R.S.

AND

MARSHALL HALL, M.D. F.R.S.

AS A HUMBLE ACKNOWLEDGMENT OF THE AID WHICH THE AUTHOR

HAS DERIVED FROM THEIR LUMINOUS RESEARCHES INTO

THE PHILOSOPHY OF THE NERVOUS SYSTEM.

PREFACE.

When great principles are invaded by error, it is not less the duty, than the right, of the conservative portion of society to come to their protection; nor can error complain that it is not permitted to move on unmolested. It must be ever willing to take its chance in a conflict with truth.

The tendency of the labours now in progress in organic chemistry to the subversion of physiological science, and therefore of pathological and therapeutical principles, and, as another necessary consequence, of rational practice, induces me to persevere in contributing my humble efforts to counteract the influence of the iatrochemical philosophers. However laudable may be their motives, and however they may astonish us with revelations in inorganic chemistry and multiply the sources of human happiness, they are nevertheless employed, in their interpretation of "facts" relative to organic chemistry, in opposition to the experimental results which have been forever in undeviating progress in every individual of the organic kingdoms. The enchantments, however, of a fascinating pursuit, and an imperfect acquaintance with those profound institutions of nature which are entirely foreign to the laboratory, and which can only be known through the accumulated inquiries of ages into both the natural and morbid phenomena of organic beings, must be allowed the weight of an apology. But, an attempt to overthrow the experience of the past, and to obscure what is written in the most legible characters, and, as it were, in thousands of languages, upon the tablets of organic life, by a distorted construction of "*facts*" which are yielded by test glasses and crucibles, appears to me to be an enterprise which should alarm physiologists as to its pernicious consequences.

That we are on the eve of some vast convulsion in physiology, can no more be doubted by those who watch the signs of the times, than that the declaration has been promulgated, *ex cathedra*, that "medicine is now in its infancy," or that efforts were made to establish a spurious system upon that assumption.

> "*Animal* and *vegetable* physiologists," says the able Liebig, "institute experiments without being acquainted with the circumstances necessary for the continuance of life, — with the qualities and proper nourishment of the animal and plant on which they operate, — or with the nature and chemical constitution of its organs. These experiments are considered by them as convincing proofs, whilst they are fitted only to awaken pity." — (LIEBIG.)

The question then is, whether "*animal* and *vegetable* physiologists" will continue to be satisfied with the "experiments and the *pity*" of the *chemical* physiologists? How this question will be decided, I can entertain no doubt if the reader will attentively examine the article on "Poisons, Contagions, and Miasms," in Liebig's Organic Chemistry applied to Agriculture and Physiology, extending from page 329 to 384; — which is certainly the most stupendous exhibition of perverted facts, of combinations of conflicting doctrines, and of the rudest system of pathology and therapeutics, that can be found in the records of dreamy speculation. And yet is Liebig profound and rich in *chemical philosophy*, and in its application to the useful arts.

I will take this opportunity of making a few explanatory remarks relative to a communication from the Editor of the British and Foreign Medical Review, which appeared in an April number of the Boston Medical and Surgical Journal, since they are necessary to a just understanding of the case.

Dr. Forbes having refused to impart to the public the name of any individual as the author of the "plagiarisms" in his Journal, and having stated that he had "sent the name of the real author of the plagiarisms to a mutual friend in New-York, with permission to mention it to Dr. Paine in confidence," it seems indispensable that I should say,—that this communication was not made to me until a considerable time after the publication of my Reply to Drs. Forbes' and Carpenter's Circular Letters, and that it was then done with the strictest injunction of secrecy, and with the information from Dr. Forbes, that the individual had gone to *Australia*. The reader should be likewise in possession of the fact that I have received information from a sure source in Europe, that Dr. Forbes has stated that Dr. Carpenter wrote a part of the Review of my Commentaries, and the "plagiarist" the other part! (1)

I would also add, that the author of the Examination of Reviews has never "*sought to crush*," as assumed by Dr. Forbes, "*with the thunders of his gravis-epistola, the British and Foreign Medical Review, and all belonging to it;*" but simply to protect his own rights, and to administer, as far as might be, exact justice to all. And now, in conclusion of the whole subject, and in consideration of the honour which Dr. Forbes has done me of republishing in his late communication a brief commendatory notice of his Journal which was granted by me on application, I will do myself the farther honour of

(1) Did the "Plagiarist" write *his* part of the review of my "Commentaries" before or after his migration to Australia?

saying, that, when Dr. Forbes communicated to me the name of the foregoing inhabitant of *Australia*, he also conveyed an invitation to enrol myself as a Reviewer in the "British and Foreign Medical Review;" a privilege of which I intend to avail myself should Dr. Forbes become an author.(1)

New-York, *June* 12, 1842.

---

(1) Perhaps it is admissible to show, that, whilst my labours were undergoing misrepresentations in London, they were treated with magnanimity in a country which is preëminent in medical philosophy, as in all other learning, which is foreign in its language and political institutions, and where I had no friend at court, — not even an acquaintance.

The following extract from the Boston Medical and Surgical Journal of June 8th, embraces a statement of the honour to which I refer :—

"In the printed catalogue of the members of the Prussian Medical Society for June, 1841, *(Verein für Heilkunde in Preussen,)* is the name of Martyn Paine, M. D. of New-York, extensively known in the medico-scientific world, for his indefatigable industry in medical literature. This Society embraces the élite of the medical faculty of Prussia and Germany, there being but one hundred and fifty ordinary members — and till the election of Dr. Paine, it had not conferred the honour of a diploma on a single person in America."

ESSAYS ON VITALITY,

AS CONTRADISTINGUISHED FROM

CHEMICAL AND MECHANICAL PHILOSOPHY.

"THESE difficulties, the solution of which gives medicine its highest character as a science, can be adequately conceived by the medical man alone. Neither those accustomed to legal evidence only, nor such as have pursued physical science in its more simple material forms, can rightly apprehend the vast difference made by the INTRODUCTION of the PRINCIPLE of life, or, yet more, of the states and phenomena of mind, in connexion with bodily organization. We have here a NEW WORLD of relations, occult and complex in their nature, to be reasoned upon and resolved; with a principle of change, moreover, ever operating among them, which makes all conclusions liable to a new source of error. It is the want of this *right understanding* of medical evidence, which makes the mass of mankind so prone to be deceived by imposture of every kind; whether it be the idle fashion as to particular remedies; *or the worse, because wider deception of some system, professing to have attained, at once, what the most learned and acute observers have laboured after for ages in vain.*" — HOLLAND. *Medical Notes and Reflections*, p. 2.—1840.

"Until it is proved that the FORCES which, in a living body interrupt the play of the natural chemical affinities, *maintain a proper temperature, and preside over the various actions of organic and animal life*, are analogous to those admitted by natural philosophy, we shall act consistently with the principles of that science, by giving distinct names to those TWO KINDS of forces, and employing ourselves in calculating the DIFFERENT LAWS they obey." — ANDRAL. *Pathological Anatomy, vol. i*, p. 422.

"Physiology would have made a much greater progress, if all those who studied it had set aside the notions which are borrowed from the accessory sciences, as they are termed. But these sciences are not accessory; they are wholly strangers to physiology, and should be banished from it wholly." "To say that physiology is made up of the *physics* of animals, is to give a very absurd idea of it. As well might we say that *Astronomy* is the physiology of the stars." — BICHAT. *General Anatomy, &c.*

"We would be glad to see the science of physiology based upon a more extensive generalization of the phenomena of VITALITY than has usually been thought necessary." — BRITISH AND FOREIGN MED. REV., *vol. iv*, p. 285, &c.

"In endeavouring to explain any of those phenomena (of organic beings) which have hitherto evaded research, it may be anticipated, that if such inquiries be successful, they will not lead to the discovery of any new laws, but unfold the same simplicity of means for performing those operations of the economy which have already been discovered." — WARDROP. *On the Nature and Treatment of Diseases of the Heart, Part I*, p. 13.

"Hippocrates totam suam philosophiam in hoc posuit, quod res congeniæ se mutuo appetant, dissimiles vero a se invicem recedant." — KEILL. *Med. Stat. Brit.* p. 32.

"The establishment of just principles, besides being the proper end of observations and facts, is certainly what confers upon them their highest value." — SENAC. *On Fevers.*

"The actions of living beings, whether normal or abnormal, are as amenable to general laws as those of inert matter; and the discovery of these laws is within the reach of those who search after them in the right track." — BRITISH AND FOREIGN MED. REV., *vol. vi*, p. 110.

"Although throughout nature nothing really exists but individual bodies having simple individual actions, according to a law, (edentia actus puros ex lege;) yet in every thing, that law, and the tracing, finding out, and explaining it, is the foundation of science and experiment." — BACON. *Nov. Organ. l. ii, Aph.* 2.

"Too many facts crowd the memory without advantage, any farther than they lead us to establish principles." — HUNTER. *Lectures on the Principles of Surgery*, p. 8.

"We have had workmen enough to toil in the mine and quarry. They have raised and roughly fashioned an abundance of materials." — LAWRENCE. *Lectures on Physiology*, p. 65.

"I have done my part by mentioning the indications to be considered, and pointing out the time and manner of doing it; for the practice of physic consists chiefly in being able to discover the true curative indications, and not medicines to answer them; and they who have overlooked this point, have taught empyrics to imitate physicians." — PREFACE *to* SYDENHAM'S *Works.*

"Those who are incapable of generalizing seek in books for particular details only. Every author, therefore, who does not tell them all they are ignorant of will appear to them unworthy of being read." — ZIMMERMANN. *Experience in Physic.*

"Theory is only common sense applied to calculation." — LAPLACE.

"We must never forget that it is *principles*, not phenomena, *laws*, not insulated independent facts, which are the objects of inquiry." — HERSCHELL'S *Discourse, &c.*, p. 13.

"A single just principle in our science will lead to more truth, in one year, than whole volumes of uncombined facts will do in a century." — RUSH. *Medical Inquiries, vol. iv*, p. 43.

I PURPOSE, in this paper, stating some original views relative to the doctrines of life as contradistinguished from those which respect inorganic nature. These doctrines lie at the foundation of all medical philosophy, of all practical medicine.

I shall proceed, in the first place, to carry out the illustration of an important, and fundamental principle of nature, as briefly set forth in my "Examination of Reviews." This exposition occurs at the 33d page of that work, and consists in a deduction of the doctrines of the vitalists from the phenomena that attend the development of the incubated egg.

It is also one of my present objects to extend the application of the foregoing principle, and to consider,

1st. The constitutional nature of the ovum.

2d. To show farther by the philosophy of generation, and by the nature of the powers which are universally admitted to be alone concerned in developing the germ or ovum, and in forming the organs of the new being, that the *same* powers are, also, *alone* concerned in carrying on forever afterwards the processes of life, and, of course, that no new powers, or principles, are introduced.

3d. To consider the manner in which the germ is impregnated, or its vital properties so stimulated into action, as to result in the development of the germ, and in unfolding the various attributes of the new being.

4th. To show that we may find in the physiology of generation, or the principles through which the ovum is impregnated, the whole philosophy of organic life, or the principles through which the actions of life are forever carried on.

5th. To state the manner in which the natural peculiarities of each parent, whether as it respects the properties of life, or the physical conformation, are infused into the germ and combined in the full-grown offspring.

6th. To show that hereditary diseases are transmitted in the same way as those more natural peculiarities which belong to parents.

7th. To show, also, that the principles which are concerned in the transmission of hereditary diseases are the same as concur in the production of ordinary diseases.

8th. To deduce from the philosophy of generation the vital nature of hereditary diseases; or, in other words, to show that the morbid impression is established upon the vital properties of the ovum, and of course, upon those of the new being; and that the hereditary vitiation does not consist in any transmitted impurity to the blood or other fluids of the offspring, as is now supposed by the humoralists, and the fallacy of which I have endeavoured to establish in my Essay on the "Humoral Pathology."

In considering the questions before us, we are but employed in tracing nature into one of her thousand labyrinths, whose exploration only requires us to avoid speculation and take nature for our guide; and here it must be our first object to interrogate her as to the constitution of the ovum, that we may ascertain by what powers its development is accomplished. At this elementary step of our being, no philosophical mind can doubt that all the component parts of organic beings are wholly different from the ordinary combinations of chemistry, and, therefore, that they are effected by powers with which chemistry has no connexion. Such being granted, analogy does the rest for the whole organic fabric.

If we go to the elementary constitution of organic beings in their state of maturity, we shall at all times find in the *modes* and *proportions* in which their elements are united, the same fundamental proof that they are combined and held in union by properties and laws peculiar to beings so constituted; and what is thus at the very foundation of organic compounds, must preside over all subsequent developments and actions.

Let us hear the illustrious Müller upon the elementary constitution of organic beings, who, in inclining to the doctrines of the chemists in his interpretation of the general results of organic processes, will be allowed to have been prompted alone by a love of truth in a statement so adverse to his subsequent conclusions, and by convictions that could only have flowed from the most elaborate inquiry.

"The peculiarity of organic matter," says Müller, "depends probably on the following circumstances, first pointed out by Berzelius and Fourcroy:

> "1st. In mineral substances the elements are always combined in a binary manner; thus, two elementary substances unite together, and this binary compound unites again with another simple substance, or with another binary compound." "In minerals the elementary substances are never observed to combine three or four together, so as to form a compound in which each element is equally united with all the others. This, however, is universally the case in organic bodies. Oxygen, hydrogen, carbon, and nitrogen, the same elements which by binary combinations formed inorganic substances, unite together, each with all the others, and form the peculiar proximate principles of organic beings. These compounds are termed ternary, or quaternary, according to the number of elements composing them." (MULLER's *Elements of Physiology, vol.* 1, p. 2.)

It is true, certain organic chemists having seen that such a constitution of organic nature would be fatal to the whole chemical philosophy of life, have not only rejected the doctrine by the force of analogy as derived from the composition of oxalic acid, and one or two other compounds which are themselves obtained by direct chemical agencies, and therefore more or less artificial, but, as we have seen on a former occasion, they have assumed the prerogative of Almighty Power in the creation, not only of organic compounds, but of the entire animal; whilst one of the most eminent gravely entertained a society of philosophers in Edinburgh with the discovery of a process for transmuting cyanogen, (compounded of nitrogen and carbon,) into the simple element, silicium. Müller notices and rejects the assumptions of the chemists relative to the few compounds which they declare to be organic, and yet composed of only two elements. Of urea, he very justly remarks, that "it can scarcely be considered organic matter, being rather an excretion than a component of the animal body." I shall only add upon this subject, that however urea may be obtained, either from the urine or other compounds, its elaboration, whether spontaneous or artificial, is not a proof of its previous existence in that particular condition; whilst the agencies employed for its production necessarily involve important changes in the original compound. This principle is obviously more or less applicable to all the chemical experiments which are instituted upon organic compounds.

I shall not advert to the remaining contradistinctions between organic and inorganic beings, beyond what relates immediately to the specific inquiry before us.

> "*The only character*," says Müller, "*that can be possibly compared in organic and inorganic bodies, is the mode in which symmetry is realized in each.* (MULLER's *Physiology*, p. 21.) Tiedemann, too, was right in saying that, "all the qualities of organic bodies should be looked upon as the effects of the vital powers. Even those phenomena seen in them, which they exhibit in common with inorganic bodies, undergo modifications of their specific action, and should be considered subordinate to the vital powers."

But, we are now especially interested about the opinions of philosophers as to the entire dependence of the development of the ovum upon a vital principle or vital properties; for if this point be universally conceded as a matter of course, then it will follow that it is the same principle or properties which are forever afterwards concerned in organic processes, and *alone* concerned. Tiedemann goes on thus:

> "It is the *vital power*, which in the fecundated germinative liquid, brings the

molecules of the organic combinations to the solid form, and calls the first lineaments of the vegetable and animal embryo into existence. All the parts and tissues that are formed in it, according to a definite order of succession, are products of the power of formation, and on this they depend in all that relates to their first appearance, their development, aggregation, configuration, and arrangement. The phenomena exhibited in the act of formation of an embryo, are placed far above all the mechanical and chemical acts we observe in bodies not endowed with life," — (Tiedemann. *Comparative Physiology*, p. 185.)

"The *creative* force," says Müller, "exists already in the germ, and *creates* in it the *essential parts* of the future animal. The germ is *potentially* the *whole* animal. During the development of the germ, the *essential parts* which constitute the actual whole are produced." "The entire vital principle of the egg resides in the germinal disk alone; and since the external influences which act on the germs of the most different organic beings are the same, we must regard the simple germinal disk, consisting of granular amorphous matter, as the *potential whole* of the future animal, endowed with the *essential* and *specific force* or principle of the future being, and capable of increasing the very small amount of this specific *force* and *matter* which it already possesses, by the assimilation of new matter." And again he says, "this force exists before the harmonizing parts, which are, in fact, formed by it during the development of the embryo." "The vital force inherent in organic beings *itself* generates the essential organs which constitute the whole being." "The formative or organizing principle is a *creative power*, modifying matter blindly and unconsciously;" yet with such wonderful precision, that Müller also says, that "this *rational* creative force is exerted in every animal strictly in accordance with what the nature of each requires." "The vital principle," he says, is in a quiescent state in the egg before incubation." (1) (Muller's *Elements of Physiology*, p. 23.)

Roget, of high authority, maintains that

"We are not warranted in the assertion that the operations of vital chemistry are directed by distinct laws, and are the results of new agencies;" and he condemns the opinion as to "a principle of vitality" as "*a fiction of the mind*." (Roget's *Outlines of Physiology*, ch. 4, p. 64.)

But, all this, (as with every enlightened chemical physiologist,) is contradicted whenever he touches specifically upon any of the actions or other results of life. There is a perpetual recurrence of "vital powers," "vital actions," "vitality," &c., in both of the able physiological works of this author. Even in respect to the composition of animals, "the mode," he says, "in which their elements are combined is so complex as to require a long and elaborate process to accomplish that purpose; and neither the organs with which animals are furnished, *nor the powers* with which those organs are endowed, are adequate to the conversion of the materials furnished by the inorganic world into the substances required for the construction of their bodies, and the maintenance of their powers. The inorganic elements must have passed through intermediate stages of combination," in the vegetable kingdom, &c. And yet the chemical physiologists assume that *animal* compounds may be created in the laboratory!

---

(1) The applicability of the following important statement to my Essay on the "Vital Powers," and to the subject of "force," "properties," and "vital principle," as investigated in my "Examination of Reviews," (p. 8—34) is sufficiently obvious.

"Whether the vital principle," says Müller, "is to be regarded as IMPONDERABLE MATTER, or as a FORCE OR ENERGY, is just as uncertain as the same question is in reference to several important phenomena in physics. Physiology, in this case, IS NOT BEHIND the other natural sciences; for the PROPERTIES OF THIS PRINCIPLE in the functions of the nerves are nearly as well known as those of light, caloric, and electricity, in physics." (Muller. *Ibid.* p. 27.)

Let us now hear this able writer on the subject of fœtal development.

> "The utmost solicitude," he says, "has been shown in every part of *living* nature to secure the perpetuity of the race, by the establishment of laws, of which the operation is certain in all contingent circumstances."
>
> "A portion of the VITAL POWER of the parent," he says, "is employed *to give origin and birth* to the offspring, and certainly no part of the economy of animated nature is more calculated to impress us with exalted ideas of the immensity of the scheme of Providence. Nothing can be more admirable than the progressive architecture of the frame," &c. Roget ultimately describes, in his usual felicitous manner, the development of the ovum, and here we have nothing from our author but the agency of "*the vital powers.*"
>
> "The foundations of the edifice," he says, "are laid in the homogeneous jelly *by the* EFFORTS of the VITAL POWERS." "At first all the ENERGIES of VITALITY are directed to raising the fabric, and to the extension of those organs which are of greatest immediate utility;" — and so on throughout the chapter; the whole work of developing and fashioning the fœtal organs being assigned to "the Efforts of the Vital Powers," and to "the Energies of Vitality." (ROGET'S *Animal and Vegetable Physiology, vol.* 1, pp. 42, 45; *vol.* 2. p. 424.)

We have thus before us a plain statement of our necessary premises as they respect the exclusive agency of the "vital principle," or "organic force," or "creative power," or "vital properties," or "vital powers," or "*vitality*," (which-ever term may be preferred,) in carrying out the full development of the embryo.

But let us hear, also, Dr. Carpenter, who advocates the chemical doctrines of life, so far as to lay down the following principle no less than twice within six pages, and in nearly the same words. — Thus :—

"Reason," he says, "has been already given for the belief that the affinities which hold together the elementary particles of organized structures, are not different from those concerned in the inorganic world; and it has been shown that the tendency to decomposition after death bears a very close relation with the activity of the changes which take place in the part during life"! (*Carpenter's Principles of General and Comparative Physiology*, p. 140; also p. 146.)

Now the authority of such a writer, and the acknowledged head of the strictly chemical school in physiology, must be allowed to be very important when any concession is made to vitalism. Let us then hear him in the matter of the ovum.

"Organization and *vital properties*," he says, "are simultaneously communicated to the germ by the structures of its parent.(¹) These vital properties confer upon it *the means* of itself assimilating, and thereby *organizing* and ENDOWING with VITALITY, the materials supplied by the inorganic world."(¹) (CARPENTER'S *Principles*, &c., p. 138.)

Dr. Prichard is strictly of Dr. Carpenter's school, (see my "Examination," &c., p. 37,) and as the writings of the latter upon the vital principle are but a reiteration of those of the former, I have hitherto thought it unnecessary to cite the language of both, and have preferred Dr. Carpenter's as the latest exposition of the strictly chemical and physical creed. But, there is a point of agreement which is worth noticing in its connexion with the subject now before us, and to which I formerly referred, in its relation to Dr. Carpenter. Both of these writers see so much of peculiar design in organic nature, that they find it impossible to interpret the phenomena of organic beings upon the chemical and physical principles which they have so strenuously put forth, and, in the end assign them to the *immediate* action of the Deity. But, let us have an example to our direct purpose. Thus:—

"The theory of a vital principle," says Dr. Prichard, "has been applied in a different manner, to account for the phenomena displayed at the beginning of life in animals and vegetables, and to get rid of the mystery which attends the gradual evolution of organic structure from ova and germs. Here the vital principle is no longer considered as a chemical agent, but assumes the character of a plastic or formative power," &c.

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(1) And yet Dr. Carpenter says that the vital properties exist in the *elements* of matter. "A writer who can so contradict himself [and on fundamental doctrines] scarcely needs to be exposed by us."—*See Dr. Carpenter's review of my* "*Commentaries*" *and my* "*Examination of Reviews*," pp. 12, 37, 86, &c., and my "*Notice of Reviews*," p. 2.

Now, then, how does Dr. Prichard "get rid of the mystery"? In the following way, namely, —

> "We may," he says, "if we choose to do so, term *the cause* which governs the organization and *vital* existence, a plastic principle, but it is a principle ENDOWED WITH INTELLIGENCE AND DESIGN [!] It is, in fact, nothing more than the Energy of the Deity." "The development of forms, according to their generic, specific, and individual diversities, not less in the vegetable than in the animal world, can only be accounted for by ascribing it to the universal energy and wisdom of the Creator." (PRICHARD'S *Review of the Doctrine of a Vital Principle*, pp. 123, 141. — See, also, *Paine's Commentaries, vol.* 1, pp. 10, 25; and his *Examination of Reviews*, pp. 37, 41, 43—44.)

This is a far greater admission than the vitalist can desire, — since, if the development and growth of the germ depend immediately upon Almighty Power, so must all the analogous processes of the living being at all stages of its existence. But, whilst this doctrine is utterly exclusive of all the assumed chemical agencies at all periods of life, and overlooks the analogy between the development of the germ and the subsequent processes, there can be no hesitation as to the disposition which should be made of it, without any reference to its prevaricating nature.

It will now be interesting to know the details of this work of development and growth, which is thus allowed, on all hands, to be conducted by powers utterly distinct from the chemical and physical, and in which these have no agency.

> "The development of the separate parts," says Müller, "out of the simple mass is observable in the incubated egg. All the parts of the egg, except the germinal membrane, are destined for the nutrition of the germ. The simple germinal disk is the potential whole of the future animal, endowed with the essential and specific force, or principle of the future being, and this germ expands to form the germinal membrane, which grows so as to surround the yolk; and by *transformation* of this germ, the organs of the future animal are produced. The rudiments merely of the nervous and vascular systems, and of the intestinal canal, are first formed; and from these rudiments the *details* of the organization are afterwards more fully developed; so that the *first* trace of the central parts of the nervous system must be regarded neither as brain nor as spinal marrow, but as still the *potential* whole of the central parts of the nervous system. In the same manner the different parts of the heart are seen to be developed from a uniform tube; and the first trace of the intestinal tube is *more* than the mere intestinal tube; it is the *potential* whole, — the representative of the entire digestive apparatus; for, as Baer first discovered, liver, salivary glands, and pancreas, are, in the farther progress of the vegetative process, really developed from that which appears to be merely the rudiment of the intestinal canal. It can be no longer doubted that the germ is *not* the

miniature of the future being, with all its organs, as Bonnet and Haller believed, but is merely *potentially* this being, with the *specific vital force* of which it is endowed, and which it *becomes actually* by development, and by the production of the organs essential to the active state of the *actual* being. For the germ itself is formed merely of amorphous matter, and a high magnifying power is not necessary to distinguish the first rudiments of the separate organs, which, from their first appearance, are distinct and very large, but simple. So, that the later complicated state of a particular organ can be seen to arise by transformation from its simple rudiment. These remarks are now no longer mere opinions, but facts ; and nothing is more distinct than the development of glands from the intestinal tube, and of the intestinal tube itself from a portion of the germinal membrane." (MULLER, *ibid.*, p. 23.)

Such, then, is the history of the development of the germ in birds, and in all the higher animals ; and the whole work is ascribed by physiologists of every denomination exclusively to principles unknown in the inorganic world, and wholly distinct from any of a chemical nature. They are called, indiscriminately, vital properties, vital powers, vital principle, organic force, creative force, &c., terms, as we have just seen, employed either by Professor Müller or Drs. Roget or Carpenter, to distinguish the principle, or properties from every thing that has any known existence in inorganic substances, or as the source of any inorganic results. But, physiologists of the chemical school stop here, and ascribe all organic compounds after the being is fully formed to chemical agencies. It is remarkable, however, that it has not occurred to these philosophers, that precisely the same elementary combinations, the same formation into tissues, and the same secretions take place at all stages of the elementary development as at all future periods of life, and that the rudimentary development consists in those formations of simple compounds and their union into tissues ; and if the early or rudimentary growth of the being, all its secreted products, all its elementary combinations, be determined by the vital properties, so are the *same* results determined by the same properties or powers forever afterwards. To call in the agency of chemical or physical forces, to accomplish *precisely* the same results at any future stage of the organic being as are admitted to be performed in the development of the "essential parts" of that being by the vital principle or vital properties alone, is not only a violation of the plainest rule in philosophy, but of the clearest facts.

We have thus before us a peculiar order of powers by which the organic being is developed, fashioned, and forever exclusively governed. It is these powers, about which physiology, pathology, and therapeutics, are essentially concerned. We may, therefore, seek in the composition of organic beings, and in the laws of their development, for the whole rudimentary principles of medicine. The vital principle has also the extraordinary task of laying out, in the ovum, the whole organization of the future being, — so that its subsequent labour must be comparatively simple; and it is then, least of all, that it can require any help from the forces of the inorganic kingdom, or that it would permit a violation of the great principle in nature, of avoiding an unnecessary multiplication of causes.

It may be farther shown, by the incipient development of the ovum, that the vital powers, or properties, are more concerned in the growth, nutrition, and all the subsequent physical results, throughout the whole existence of the being, than is generally supposed by even the exclusive vitalists. The usual supposition is that the vessels or instruments of action, which are moved by the vital powers, perform the work of decomposing the blood and other parts, and recombining them again in other proportions and forms, according to the particular organization of parts, and the modification of their vital states. (1) But according to all physiologists, the ovum, in its germinating part, is a mere organic fluid, destitute of vessels, and all other physical parts of the future apparatus.

> "That the form," says Müller, "of the organic matter does not determine originally the mode of its action is proved indisputably by the fact, that the matter from which all animal forms are produced is at first almost without form." "The germ is a round disk of simple animal matter," &c.

The first "assimilation of new matter," therefore, must take place without the agency of vessels, or any of the parts which are subsequently formed; and, therefore, the same powers which converted the fluid germ into vessels, nerves, &c., continue to make the same conversions out of blood; and as all this was originally done without the aid of vessels, so must the same process be forever carried on without their *direct* agency. Nothing in mathematics can be more certain, and nothing, therefore, is more incontrovertible. (*See the last note.*)

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(1) It is affirmed, however, by a very eminent chemical physiologist that, —

"The most determined skeptic cannot assert that there is any necessary relation, or, indeed, *any relation whatever* between the mechanical arrangements and the chemical properties to which they administer. There is *no reason* why the chemical changes of organization should result from the mechanical arrangements, by which they are accomplished [!] neither is there the *slightest reason* why the *mechanical arrangements*, in the formation of organized beings, *should lead to the chemical changes of which they are the instruments*"! — (*Dr. Prout. Bridgewater Treatise, ch.* 4.) Such is the proof which chemistry offers.

By the same rule it may be at once shown that the only ingenious chemical hypothesis ever invented to interpret organic results, — the *catalytic*, — is purely an assumption; since this hypothesis is predicated of the blood vessels. But, there being no vessels in the germ, the first vessels must, of course, be produced without the supposed chemical influence of vessels, and by my showing, therefore, as to the subsequent formation of vessels and other parts, the supposed agency of catalytic forces is a mere assumption. (*See Med. and Phys. Commen.*, vol 1, p. 74—76.)

By thus reducing this question, which is apparently surrounded with difficulties after the animal acquires its complex structure, to its simple elements, the obscurities disappear; since what is true of the development of the germ must be equally true of the *same* processes and results in after life.

The question then arises as to what is the particular office of those vessels where the elementary combinations and decompositions take place? Simply this, to convey, and eliminate through the agencies of the vital properties, those parts from the blood out of which the vital properties effect the new elementary combinations, whether solid or fluid, — to aid in arranging the new molecules, — or to carry forward those fluid products which may be destined for other ends.

But have not the nerves an indispensable agency in effecting the elementary combinations and decompositions? Certainly not, for the reasons which I have now stated in relation to the vessels; and this induction concurs with all other facts, and overthrows the hypothesis that the nerves are conductors of galvanism, and, therefore, the supposed agency of this fluid in the processes of life.

But all the vessels, and all the solid parts of the organism, have their various specific offices. Here, in every part, reside the vital properties, which had been fully developed in the ovum, and here are they modified according to the exact nature of the organization and the peculiar final causes of "the properties of the vital principle" in each part. Hence they manifest peculiarities in parts that are nearly analogous. The modifications vary, for instance, in the serous membranes, and more remarkably in the mucous, as known by the influence of foreign agents, their phenomena, their products, &c. The vital properties differ in different parts of one and the same continuous tissue, as in the mucous membrane of the nose, of the stomach, and of the intestines. Hence, one of the important objects of studying the structure of organs, and the nature of their tissues; for as the vital properties are naturally modified in different parts, so will their alterations in the same disease be different in different tissues of one organ, and for the same reason, even in different parts of one continuous tissue, as in the alimentary mucous membrane, where inflammation of the nose, of the stomach, and of the intestines, is distinguished by almost as striking peculiarities in the vital signs as are the physiological functions of the different organs.

These natural modifications of the vital properties in different parts have, at least, three great final causes. The first is what I have already stated, namely, to separate from the blood, through the agency of the capillary vessels, that exact part which is to be decompounded at any given point; the second is, by these modifications, to enable the "properties of the vital principle" to decompound and recombine the elements according to the exact nature of the combinations which belong to the part; and the third, to qualify the properties, through the medium of the capillary vessels, to shape and unite the new molecules to the old. It is easy to apply this principle, under its different aspects, to all other vessels, as the veins, the secretory and excretory vessels of the glands, and the absorbents.

At the first start of the development of the germ, "the properties of the vital principle" are not aided by any of the foregoing physical means, though they come into operation at the moment they are successively produced. "The properties of the vital principle," therefore, must exist in that organic fluid, in a modification, and with a formative energy which they do not possess in any of the new developments.

There are some branches of physiology of which chemistry has taken more complete and unresisted possession than of others, and the entire philosophy of digestion is almost by common consent consigned to the laboratory. Having, on former occasions, endeavoured to expose this perversion of nature, I will here add an illustration of the vital character of digestion by regarding that phenomenon in connexion with the principles now before us.

The apparent difficulty with the chemist, (who lives, of course, in great practical ignorance of organic nature, and biased by his pursuits in the inorganic world, or as dead organic matter may come under the influence of fire and acids,)—the apparent difficulty, I say, with the chemist has been in conceiving how such a fluid as the gastric juice should possess life enough to effect the transformation of all sorts of food into chyme, or how this remarkable process should be accomplished by any other power than such as he is accustomed to witness in the remotely analogous results of chemical affinities; and yet he meets the stubborn fact that, in all animals, whatever the nature and variety of food, the chyme is the same compound in all; one unvarying homogeneous substance throughout. (*See Digestion, in Comm.* vol. 2, p. 78—122.)

But it is only my present purpose to say, that we have in the development of the ovum all the necessary elements to interpret the philosophy of vital digestion by the gastric juice. This is in all fundamental respects analogous to the physiology of the impregnated fluid ovum, with the specific difference, that the elementary combinations and the modified properties of the germ are adapted to the creation of a new being out of itself and the blood which is transmitted to it by the viviparous parent; whilst those of the gastric juice are constituted for preparing or "assimilating new matter" for the ultimate growth and repair of that being. We may notice, therefore, the following points of analogy.

1st. The germ is an organic fluid, and so is the gastric juice.

2d. The germ possesses "a creative force" by which new matter from without is assimilated. Here the analogy is very remarkable, since the gastric juice in bestowing the first vitalizing act upon dead matter must also be peculiarly "*creative.*" In adopting this expressive term, however, from Müller, I would say that both that philosopher and myself employ it in a purely physiological sense, as implying a power impressed by the Creator upon organic beings of perpetuating individuals exactly similar to such as were originally the direct work of Omnipotence; but utterly reject the atheistical doctrine of spontaneous generation, and its kindred one which supposes that animals of superior organization may produce an inferior order. (*See Spontaneous Generation, in Comm. vol.* 2. *p.* 130, &c.)

The germ in all animals, so far as known, "is a round disk of simple matter, having no difference of form corresponding to the difference of animals." (*Müller*, *p.* 26.)

Nature, however, has in contemplation the elaboration, out of this germ, of various fabrics; but all upon a common plan in respect to organic life. She has therefore constituted the sensible conditions of the germ in a manner so nearly alike in all, that its differences in different species, in its elementary combinations, and in its sensible form, cannot be detected.

Just so in respect to the gastric juice. It is destined to convert all kinds of food, in all animals, into one homogeneous substance, apparently alike in all, because the elementary combinations of all animals are nearly alike. The gastric juice, therefore, like the germ, appears to be the same to the chemist in all animals; as does also the chyme.

And now what says our great organic chemist as to the agency of a *vital principle* in the most essential part of digestion?

> "The equilibrium," he says, "in the chemical attractions of the constituents of the food is *disturbed* by the VITAL PRINCIPLE, as we know it may be by many other causes. But the *union of its elements*, so as to produce *new* combinations and forms, indicates the presence of a *peculiar mode of attraction*, and the *existence of* A POWER *distinct from all other powers of nature*, namely, A VITAL PRINCIPLE." (LIEBIG. *Organic Chemistry in its applications to Agriculture and Physiology*, *p.* 357. 1841.)

The process of generation presents a varied and most impressive illustration of the peculiarities of the vital properties, and of the manner in which they are liable to be impressed and permanently modified in their nature. It results in the production of organic beings similar to those which exercise the generative faculty. This faculty is therefore manifested with as many specific modifications as there are different species of organic beings. If we allow to the globe one million of distinct species of animals, the specific modifications of the germinal product will be as numerous, and these are more or less influenced by the semen of the male. The seminal or productive principle of the male exerts its special influences upon the living properties of the germ, and *directs* their operation in such a manner that none but beings of the same kind with the parents are produced. That the various modifications which distinguish each species are determined by both parents is fully demonstrable in hybrid animals, and is sufficiently obvious in the transmission of the peculiarities of the male, where the individuals are of the same species. And notwithstanding our supposed million of distinct species of animals, and the specific variations in all the parts of each species, (which varieties in animals and plants may be safely estimated at fifty millions,) this almost endless variety is made up by successive depositions of elementary compounds out of mainly four simple substances, namely, oxygen, hydrogen, nitrogen, and carbon, three of which are gaseous, united in modes unknown to chemistry, and which chemistry cannot detect, and forever uniting in different modes and proportions according to the exact nature of every part. The act of generation establishes the modifications which are to be continued, without variation, through the life of the new being; and this new individual, becoming in its turn the agent of procreation, perpetuates all the specific modifications which appertain to itself and to its ancestors. The intermingling of species, which results in hybrid animals, proceeds upon the same plan. It must therefore necessarily be, that the vital properties of the ovum are so impressed by the exciting properties of the semen, that those peculiar elementary combinations and aggregations are started which ultimately make up the hybrid. "These vital properties," says Dr. Carpenter, "confer upon it the *means* of itself assimilating, and thereby *organizing*, and *endowing* with *vitality*, the materials of the inorganic world,"—leaving it, also, clear to all minds that the action of the semen must be exerted directly upon the vital properties of the ovum.

That this important question as to the direct action of the semen upon the vital properties of the ovum, and its capability of establishing certain modifications of these properties, and that the humoral interpretation of transmitted peculiarities is an unfounded assumption, may be definitively settled, I will also add

"The well-known fact, that when the Earl of Morton's Arabian mare was covered by the quagga, not only did the mule so begotten partake of the characters of the sire, but when the mare was subsequently submitted to an Arabian stallion, by whom she had three foals at different times, the first two continued to exhibit some of the distinctive peculiarities of the quagga conjoined with the characters of the Arabian breed."—(MONTGOMERY, *on the Signs and Symptoms of Pregnancy*, p. 17. London, 1837.)

The profound writer whom I have thus quoted, goes on to state the true philosophy of this phenomenon, in supposing that the semen "may influence several ova and so continue to manifest its effect in the offspring of subsequent conceptions when impregnation has been effected" by males of another species.

The semen, then, is a vital stimulus, and so far on a par with the ordinary stimuli of life. These may be natural, like air, heat, food, &c.; or they may be morbific, like malaria, poisons, &c.; or curative, like medicines. In all the cases, their action is upon the vital properties; and it is in consequence of these influences, that the ovum is developed, that life is maintained, health preserved or impaired, or disease removed. The ova of oviparous animals show the analogy in respect to stimuli, and the principles involved, more impressively than those of viviparous; since by an admirable design, in respect to the former, the impression of the semen has a limited operation, when the vital properties of the ovum return to their quiescent state, but may be again roused into action by the simple stimulus of heat. (*See Comm. vol.* 1. *p.* 21, &c.)

The action of the semen upon the properties of the vital principle of the germ is a type of all the influences that are produced upon the same vital properties during the life of the animal, and from which all its organic actions, and all their results, arise. This is the whole essential philosophy of physiology. It is the alterations produced in the vital properties which constitutes the philosophy of disease, and in which, indeed, all disease virtually consists. It is the art of finding out the remote causes, and the nature of the alterations they produce, and of adapting to the altered condition of the properties of life such agents as shall establish new impressions upon them, and thus enable them to return to their natural state, which forms the basis of therapeutics in its connexion with pathology.

Let us now see if the beginning of individual existence does not supply a key to the whole philosophy of disease, as it does to that of physiology. We have seen that all the actions, and all the results of life, are merely effects which arise from the operation of the vital properties through their organic instruments, as bloodvessels, &c. Those properties must be constantly excited into action by foreign agents, as by food, blood, &c., or the properties will become extinct, and of course, the effects will cease. Now, the actions in disease are nothing more than the altered actions of health, and the same rule applies to all the morbid products. It follows, therefore, that the properties of life, upon which these altered conditions depend, are modified or altered in a corresponding manner. As a consequence of this, it also results that the vital properties have been varied from their natural state by agents or causes capable of producing the change. These agents make their impressions in the same way as the natural stimuli of life, only the morbific agents at the same time affect the nature of the vital properties, and bring them into a new condition. This new condition constitutes disease.

The type of all this may be found in the impregnated ovum. The properties which animate the germ *before* conception are entirely determined by the vital constitution of the female parent. But we have seen that the new being may partake of the physical characters of the male as well as of the female, and it not unfrequently happens that the characteristics of the male are predominant. Hence it follows that the semen so far establishes changes in the original constitution of the vital properties of the germ.

Since, therefore, all the fœtal developments, and all their physical peculiarities, depend upon the precise modifications and actions of the vital properties, and since these properties in the unimpregnated ovum are entirely determined by the female parent, their nature after impregnation must be more or less affected, and assimilated to the peculiar nature of the male parent in all the cases where the offspring manifest any of the male characteristics. This is entirely analogous, in principle, to the modifications which are produced in the properties of life by morbific causes, but with this difference in contingencies. In the case of the impregnated ovum, the modifications are permanently established, and can never be altered, so far as the vital properties, in either parent, upon which the modifications depend, are fundamental in their nature. In the case of the morbific agent, or the cause of disease, the vital properties are diverted from the healthy state, and from *such* modified conditions they commonly possess the ability of escape, and of returning again to their natural standard. In the case of the impregnated ovum, a modifying agent operates, whose properties are intended to confer on the new being a stable condition, however they may modify the exact constitution of the impregnated germ. This vital stimulus, the semen, therefore, in virtue of its specific properties bestows upon the corresponding vital properties of the ovum the peculiarities which belong to itself; and these being natural, vital, and determinate, the transmitted peculiarities should be equally so. In the case of disease, however, the morbific agents have none of the properties of life which are natural to the fecundating semen, and the modifications, therefore, which they may determine may be different, even if we suppose them to act, as in the case of the germ, upon the whole constitution. Whatever modifications, therefore, may arise from their action, they must consist of deviations from the standard of health. But it does not necessarily follow that certain artificial modifications may not be as permanent as the natural ones; and, it is from observation alone, that we learn that they are so.

In the case, however, of the formation of new "temperaments" by change of climate, and the more remarkable alterations which are produced in animals by domestication, the results are brought about by the new and habitual influences to which the properties of life are exposed; and, in all these cases a radical and permanent modification is established, closely approximating the modifications which are bestowed upon the germ by the fecundating semen. Now, it is also true, that what is denominated predisposition to disease is entirely analogous, in principle, to the permanent temperaments of which I have just spoken. Both are results of physical agents, modifying the properties of life; and this chain of analysis conducts us to those predispositions to disease which are impressed upon the germ by the fecundating semen, and by which we show that the philosophy of the operation of morbific causes is variously, and even exactly exhibited in the impregnation of the germ.

Take the scrofulous subject as supplying an example of hereditary predisposition to disease. If it exist in the female, her ova will partake of this peculiar modification of the vital properties, and it is in this way that her progeny inherits the scrofulous diathesis. Here, then, we see illustrated in the very ovum, even before impregnation, the whole principle which concerns artificial temperaments, and those influences of morbific agents which establish predispositions to disease in the full grown subject. It frequently happens, also, that this natural diathesis is so great, that it results in actual disease before the birth of the offspring, *as manifested by tuberculous affections in the lungs of still born infants.*

But to make the philosophy of this subject more obvious, let us consider the germ when it derives its scrofulous disposition from the male parent. Before impregnation, its vital condition is perfectly natural. The semen of the male parent establishes upon it the modification which constitutes the predisposition to scrofula, just as malaria determine those modifications which result in fever, &c.

The subject may be pursued under a variety of aspects, and with various illustrations, whether physiologically, or pathologically. The same principle is concerned throughout, whether in respect to the physiological conditions impressed upon the ovum by the seminal fluid, or as these conditions are modified in hereditary scrofula, gout, &c., or whether it concern the temperaments and other permanent changes that are induced by climate, domestication, &c., or, as malaria may establish their peculiar modifications of the properties of life. Nor can such conclusions be unexpected to those who duly consider the simplicity of nature in her elementary principles and laws.

Could the doctrine entertained by Walker, Elliotson, and others, that the imagination of the parents influences the physical organization of the offspring, the philosophy which I have set forth, though not rendered more clear, would be yet fortified. But, that is at best but speculation. I could, however, turn to the mysterious production of the soul. This remarkable principle is doubtless developed at the very outset of fœtal life, from the circumstance of its often combining the intellectual peculiarities of both parents, or again of manifesting chiefly those of the male. But here we have no other fact to guide us, and all beyond has been involved in an impenetrable mystery by the great Creator. Here it is a pride and a help of philosophy to rest on faith alone. (*See Commentaries, vol.* 1, *p.* 82—106.)

CONCLUSIONS DERIVED FROM THE PRESENCE OF NITROGEN IN ORGANIC COMPOUNDS.

"It is found that no body destitute of nitrogen possesses, when pure, the property of decomposing spontaneously whilst in contact with water."

"Every azotized constituent of the animal or vegetable organism enters spontaneously into putrefaction when exposed to moisture and a high temperature."

"The VITAL PRINCIPLE opposes to the continual action of the atmosphere, moisture, and temperature, upon the organism, a resistance which is, in a certain degree, invincible." LIEBIG. *Organic Chemistry in its Applications to Agriculture and Physiology, pp.* 257, 357. *London:* 1841.

I shall now present one or two objections to the chemical philosophy of organic life, as they are founded upon facts in chemistry, and as I am desirous of bringing them to the support of what I have advanced, at other times, upon this topic. The views, I believe to be original with myself.

It is abundantly evident that living organic beings are endowed with properties which protect their elementary composition against all those decomposing agencies which are perpetually separating the elements of all mineral compounds. This shows that the properties, by which the elements of living beings are united, are utterly different from such as combine the elements of inorganic compounds. Nevertheless, the living organization is undergoing a systematic change, a perpetual decomposition, surpassing any mutations that are in progress in the surrounding world. These decompositions are, also, of a peculiar nature, governed by established laws, various in different parts of the same individual, yet forever the same in any given part. I shall not stop to show how the old are replaced by new materials, and how the processes go on *pari passu*, and in opposition to all the philosophy which chemistry teaches, but only say that the decompositions must be effected by properties as peculiar to the living compound as are the results; and that these results conspire with the peculiar modes in which the elements are combined in proving the existence of specific properties, which are the common cause of all the harmonious phenomena of living beings.

When, however, the organic being dies, a new order of decomposition begins, eminently of a chemical nature, and in forcible contrast with that which concerns the vital process of renewal. Organic beings, indeed, contain within themselves a special principle of dissolution. It is incorporated in the very nature of the elements, and in the modes in which they are combined; being combined in such modes as facilitate their transformations and disunion after the properties of life have lost their sway. The moment these cease, chemical decomposition begins,—confusedly, violently; and such are the nature and combinations of the elements, that their disruption would go on with no other contribution from surrounding agents than water alone.

This brings me to my principal object in resuming this inquiry, which is to show that besides the modes in which the elements of organic beings are combined, one of the elements contributes vastly to the chemical decompositions which organic bodies undergo as soon as the vital principle becomes extinct.

This element is that anomaly in the inorganic kingdom, NITROGEN GAS, which abounds especially in animals. Liebig says of it, that,—

> "There is some peculiarity in the nature of nitrogen, which gives its compounds the power to decompose spontaneously with so much facility,"—as in the processes of fermentation and putrefaction. "Now, nitrogen is known to be the most indifferent of all the elements. It evinces no particular attraction to any one of the simple bodies, and this character it preserves in all its combinations; a character which explains the cause of its easy separation from the matter with which it is united." And again, "when those substances are examined which are *most prone* to fermentation and putrefaction, it is found that they are all, *without exception*, bodies which contain nitrogen." LIEBIG's *Organic Chemistry applied, &c., p.* 241.

In the inorganic kingdom, nitrogen is mostly confined to the atmosphere, where it probably exists in a state of simple intermixture with oxygen. "All bodies which have an affinity for oxygen abstract it from the atmosphere with as much facility as if the nitrogen were absent altogether;" and we have striking examples of the disposition of nitrogen to separate from its compounds, "in the easy transposition of atoms in the fulminating silvers, in fulminating mercury, and in all fulminating substances," whose ready explosion is owing to the presence of nitrogen.

Now the foregoing characters belong to nitrogen only as it exists in inorganic or in dead organic compounds. In living beings, where it abounds, it adheres to its associated elements with a tenacity which no chemical agent can impair till it destroys the life of the part; or in other words, till it destroys those vital properties by which the elements were truly united. It is then, however, that the forces of chemistry take possession, and the elements of the part may explode, I had almost said, with the facility of the fulminating compounds.

> "There is," says Liebig, "in the nature and constitution of the (inanimate) compounds of nitrogen, a kind of tension of their component parts, and, a strong disposition to yield to transformations, which effect spontaneously the transposition of their atoms on the instant that water or its elements are brought in contact with them." In speaking of the fulminating compounds, he remarks that, "all other substances containing nitrogen acquire the same power of decomposition, when the elements of water (oxygen and hydrogen) are brought into play."—LIEBIG, *ibid.* p. 244.

But, although dead animal compounds readily pass into spontaneous decomposition under slight degrees of moisture, yet composed as they are, in part, of the elements of water, and very largely impregnated with aqueous substances in their living state, neither this water nor any other agent, can disturb the exact combinations.

But when the organic being dies, chemical agencies have their play, and it is then that

> "The result of the known transformations of substances containing nitrogen proves," according to Liebig, "that the water does not merely act as a medium in which motion is permitted to the elements in the act of transposition, but that its influence depends on chemical affinity. When the decomposition of such substances is effected with the assistance of water, the nitrogen is invariably liberated in the form of ammonia." LIEBIG.

Whilst, therefore, such is the disobedience of nitrogen to all the ordinary restraints of chemical laws, as it exists in mineral compounds or in organic bodies divested of life, that it either struggles to escape, or is readily liberated by slight influences; yet, on the other hand, although entering largely and universally into organic beings, it is intimately combined in all with other elementary principles, and is restrained in its place by some power which even puts at defiance all those chemical influences that aid its escape from its elaborate combinations after they have lost the living principle. Its escape breaks up, of course, the organic compound. The hydrogen, the oxygen, and all the other elements then start from their places, and ultimately enter into those binary combinations which are peculiar to the inorganic kingdom. The nitrogen seizes upon the hydrogen, and forms ammonia, which becomes an important source of aliment to the vegetable kingdom. (1.)

But besides this disposition of nitrogen to tear asunder the elements with which it may be combined, the complexity of these elements in organic beings contributes to the disorganizing results after death, and is another principal cause of spontaneous fermentation and putrefaction.

From the foregoing facts, especially from the universality and fixedness of nitrogen in organic beings, I arrive at the conclusion that the elements of their compounds are united by forces as peculiar as the facts which relate to these compounds, and that the forces of chemistry have no agency in combining the elements, or in effecting changes of their combinations during life.

It is certainly remarkable, that nitrogen, which aids so powerfully in breaking up the elementary combinations of organic beings as soon as dead, should be made such an universal and so large an element in the organic kingdoms; and this the more so, when it is considered that nitrogen is utterly excluded from all combinations in the inorganic world. Were it consistent with proper brevity, I would inquire why this is so; but, being so, it evidently forms a strong characteristic distinction between the organic and inorganic kingdoms. We may readily suppose that among its final causes is the promotion of vital decompositions and recombinations, and ultimately of a complete resolution of the elements. And in respect to the inorganic world, had nitrogen been incorporated in its compounds, there would have been no stability among them. They would have been perpetually undergoing decomposition, until finally the whole of the nitrogen would fly off by itself, and nothing of the original compound would remain, and it could never be recombined.

Intimately, and largely combined, therefore, as nitrogen is with other elements in the organic kingdom, it irresistibly fol-

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(1) Liebig supposes that vegetables derive nearly all their nitrogen from the ammonia that arises from the decomposition of animal substances. Whence, then, did they obtain their nitrogen after their first creation, and before a supply could have been yielded by the animal kingdom? The nitrogen of the atmosphere was and is appropriated by plants; and this fact evinces their organic action upon substances in their elementary state. — See LIEBIG'S *Organic Chemistry applied to Agriculture and Physiology*, p. 70, &c., *London*, 1841.

lows that it is there united by forces essentially different from the chemical; and, when the being dies, we obtain another and opposite demonstration of the fact in its immediate obedience to chemical laws.

However the chemist may usurp the high calling of the physiologist, we shall find him perpetually baffled in his enterprise, and calling upon nature for help. In respect to the principle which maintains in union the elements of dead organic matter, Liebig says, —

> "The atoms exist only by the *vis inertiæ* of their elements. They seem merely to retain *passively* the position and condition in which *they had been placed.*" (Liebig, *ibid.* p. 236.)

Of course, then, he must necessarily allow that some other influences than the chemical originally "place the elements in a position and condition which they passively maintain" after those influences shall have ceased. In my "Examination of Reviews," (p. 16,) I have shown extensively, that Liebig refers not only the actions of organic beings, but their elementary combinations, to what he denominates a "*vital principle*," and which he considers utterly distinct from the chemical forces. It presides, in his opinion, over the whole organic economy; and he finally adds, that the

> "Vital Principle *opposes* to the continual action of the atmosphere, moisture, and temperature, upon the organism, a resistance which is in a certain degree *invincible.*" (Liebig.)

I have also quoted this principal organic chemist, in the same work, (p. 63,) on the side of vitalism, as it respects digestion; and I will add here one quotation more to show how far hypothetical views surrender to the ordeal of nature, when brought to that great test of all philosophy.

> "The equilibrium," says Liebig, "in the chemical attractions of the constituents of food is disturbed by the vital principle, as we know it may be by many other causes. But the union of the elements, so as to produce new combinations *and forms*, indicates the presence of a peculiar mode of attraction and the existence of a power distinct from all other powers of nature, namely, the vital principle." "If the food possessed life, not merely the chemical forces, but *this vitality* would offer resistance to the *vital force* of the organism it nourished." "The individual organs, such as the *stomach*, cause all the organic substances conveyed to them which are capable of transformation to assume new forms. The stomach *compels* the *elements* of these substances to unite into a compound *fitted* for the formation of blood. But the blood possesses no power of causing transformations. On the con-

trary, its principal character consists in its readily suffering transformations; and no other matter can be compared in this respect with it." (LIEBIG, *Op. cit.* pp. 356, 357, 346. — See, also, *Article on Digestion, in Med. and Phys. Comm.*, vol. 2, p. 79; and my *Examination of Reviews*, p. 62.)

The "vital principle," Liebig, of course, supposes to reside in the gastric juice, as well as in all parts of the organized being, and that digestion itself is performed by it. It is, however, the language of a chemical philosopher of life, who has embarked his fortunes upon a conflict with that very philosophy which he now and then concedes is fundamental in nature.

Vitalism becomes established in all its aspects, even in what has been denominated "transcendental vitalism," when it may be shown that the elements of organic beings are "united by a peculiar mode of attraction, resulting from the existence of a power distinct from all other powers of nature, namely, a vital principle," since the powers and laws which regulate the composition must be at the foundation of all the subsequent results. After having considered extensively, and on various occasions, the proof of the peculiar vital constitution of organic nature, and of its total abstraction from the forces and laws of inorganic matter, I have brought to the aid of my conclusions the powerful concessions of the most eminent men with whom I have had the honour to hold this discussion. It is manifest that such authorities must weigh with nearly the force of demonstration, since their concessions can only flow from convictions that have been obtained in the school of nature. Amongst the most illustrious of the adverse school of philosophers is Liebig, and standing intermediate is the profound and erudite Müller, who, I cannot doubt, will yet be shoulder to shoulder with his eminent cotemporary, Tiedemann. And having thus referred again to this great philosopher, I will not lose the opportunity of obtaining from him an important contribution to the doctrines of vitalism as they relate to the very composition of organic beings, and in which he institutes a broad contrast between the affinities which unite the elements of organic and inorganic substances. Thus:

"Chemical substances," says Müller, "are regulated by the *intrinsic* properties and the elective affinity of the substances uniting to form them. In organic bodies, on the contrary, the power which induces, and maintains, the combination of their elements, does not consist in the *intrinsic properties* of those elements, but in *something else*, which not only *counteracts* those affinities, but effects combinations in *direct opposition* to them, and *conformably* to the laws of its own operation." (MÜLLER. *Elements of Physiology*, p. 4.)

Now this "*something else*" is what we have seen Müller denominates, in the germ, the "creative force," "the vital principle," &c.

I shall conclude this article by a quotation from Tiedemann, which should be kept in view in all chemical analyses of organic substances, and in considering all their spontaneous changes after death; namely, —

> "Even when the life of organic bodies is *extinct*, we should consider the *qualities* which they possess from the time of death to the complete resolution of organization, as results of the vital powers which have been active in them." (TIEDEMANN'S *Physiology*, p, 29.)

N. B. Since "*every azotized* constituent of the animal and vegetable organism enters spontaneously into putrefaction after death," — and considering the other facts as to the intractable nature of *nitrogen* in the hands of the chemist, by what special art, when he *creates* organic compounds, does he "compel" nitrogen (*see statement as to gastric juice*, p. 33) to undergo a change of its natural characteristic, "by which," according to Liebig, "it evinces no particular attraction to any one of the simple bodies," but an absolute repulsion? Will the chemist answer? Dr. Carpenter says that "we cannot yet succeed in producing artificially any organic compound, even of the simplest kind, by directly combining its elements, because we cannot bring them together in their requisite states and proportions;" and he observes that "Dr. Prout justly remarks, that the *agency of vitality* combines them in modes which we cannot imitate." (CARP. *Princip*. p. 146.)

I say nothing of the pretended creation of *animals* in the laboratory; but refer merely to the assumption, as set forth by Liebig, that, "*by chemical agency we can produce the constituents of muscular fibre, skin, and hair*"! (LIEBIG, *ibid.* p. 354. See, also, particularly my *Examination of Reviews*, p. 40.)

Let us, therefore, weigh well the "facts" which may be contributed by organic chemistry.

ON THE MODUS OPERANDI

OF

REMEDIAL AGENTS.

"THE VITAL FORCE appears to be affected *primarily* by a great many poisons, by the vegetable or animal emanations, known by the name of miasmata, and by various modifications of the external agents which are incessantly acting upon us, as want of due exposure to the sun, too damp an atmosphere, and an unwholesome diet." — ANDRAL. *Pathological Anatomy, vol. i*, p. 423.

"It seems to me that the explanation which represents Nature always pursuing a uniform course in her operations, drawing the same results from the same principles, has a greater degree of probability than that which shows her separating, as it were, this phenomenon from all the others, in the way which she produces it." — BICHAT. *Gen. Anat. &c., vol. ii*, p. 54.

IT is the object of this Essay to inquire into the modus operandi of remedial agents.

In the first place I hold, in comformity with the proof which I have set forth in the Medical and Physiological Commentaries, that the vital principle is a real substantive agent, of which the vital properties, (irritability, mobility, &c.,) are elements, superadded to organic beings after the creation of their structure; and that the nervous power was especially superadded to man and animals, as were also the soul and principle of instinct. In what way the nervous power, the soul, and the principle of instinct, are related to the vital principle, of which the organic properties are the elements, (or as Müller has it, "*properties of the vital principle*,") we know not; but as an intimate relation does exist, it is not unbecoming human reason to attempt some knowledge of its connexion. (*See Med. and Phys. Com. vol.* 1., p. 82—93.) I also now assume, what I have endeavoured to prove in the same work, as well as in the foregoing essays, that all organic actions are carried on by the vital properties through the medium of their instruments of action, — that all vital stimuli, whether natural, morbific, or remedial, operate directly upon those properties, (or, if it be preferred, the agent makes its impressions upon the instruments of action through the medium of the vital properties,) that all disease consists, essentially, in a modification of those properties and a consequent change of function, and, finally, in the language of Bichat, —

> "It is undeniable that all remedies have for their object the restoration of the *vital forces* to the natural type, from which they have been driven by disease. Since the morbid phenomena may be considered as different *alterations of these forces*, (1) the action of remedies should also be viewed as the means by which these alterations are to be brought back to their natural type." BICHAT. *General Anatomy applied to Physiology and Medicine, vol. i*, p. 19.

I also say, with that illustrious philosopher, that if you

> "Examine all the physiological and all the pathological phenomena, you will see that there is no one which cannot be ultimately referred to some one of the vital properties of which I have just spoken." BICHAT, *ibid.*, p. 17.

In which sentiments Andral concurs:

> "In *every* disease," he says, "not immediately produced by external violence, the symptoms that occur depend either on a *lesion* of the (*vital*) *forces* (1) that *animate* every living part, or, on a lesion of organization. The *former* is *primary* and *constant;* the latter is *secondary*, variable in its nature, and *inconstant* in its existence. ANDRAL. *Patholog. Anat. vol. i*, p. 422.

I farther believe, that I have shown sufficiently, in the Medical and Physiological Commentaries, what I have affirmed in my Materia Medica, namely, that

"The partial absorption of certain remedies is only a contingent result, and has little or no agency in the physiological phenomena. Their reputed absorption is greatly overrated, often only imaginary, and sometimes misrepresented. Such as have no natural relation to the vital properties modify the natural condition of the absorbing vessels before they can enter the circulation."

Whatever is true of remedial agents, in this respect, is equally so of morbific, as I have also specifically set forth; whilst the whole philosophy of their operation rests upon physiological principles. The effects of physical causes are exactly on a par with those of a moral nature; and the cathartic which purges, the emetic which vomits, the narcotic which induces sleep, or each, and all, when they destroy, produce their effects upon exactly the same principles as the warmth which instantly rouses all the processes of life when applied to the surface in certain morbid conditions, or as offensive spectacles and odours lead to instant vomiting and purging, or to syncope, or to a paroxysm of fever; or, again, as an hour's change from one part of the town to another suspends pertussis, or as one passion cures the most obstinate maladies, or as another is instantly fatal.

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(1) See a remarkable misrepresentation as to the word FORCE, by Dr. Carpenter, in my "Examination of Reviews," p. 7, &c. Also, preceding pp. 14, 36.

The fundamental philosophy of disease is perfectly simple, as is also that which concerns its cure. Disease is essentially nothing more than a deviation of the properties of life from their natural standard, and a consequent corresponding change of functions over which the affected properties preside. The artificial cure consists in a restoration of those properties and functions by making upon the former certain impressions which enables them to obey their natural tendency to a state of health; for it should be observed that no remedies are capable of transmuting diseased into healthy conditions. That is the work of nature. Most of our medicines may, also, operate as morbific agents, and even such remedies as are not directly of this nature, exert their influences upon the same principle. The former operate, by establishing, in a direct manner, certain morbid alterations in the already diseased properties and actions of life, which are more conducive to the natural tendency that exists in the vital properties to return from morbid to their natural states. The latter also exert effects which are analogous, since these, in like manner, do but lead to changes in the diseased conditions which enable them to take on the recuperative process.

The *vis medicatrix naturæ*, therefore, consists in an inherent tendency in the properties of life to maintain their state of integrity, or to return from those altered conditions which constitute disease to their natural state, or a state of health. When, therefore, disease subsides under the influence of remedial agents, it is only in consequence of that great law, which is brought into sensible operation by the production of morbid states which are favourable to its development; but if disease terminate fatally, it is owing either to morbid alterations which transcend the recuperative tendency, or to physical obstacles which have resulted from the altered vital conditions. If disease subside spontaneously, it arises from the operation alone of that natural principle which has been established for the preservation of health, and the perpetuation of organic beings. Of this we have remarkable and striking examples in smallpox, measles, &c.

But, for wise purposes, a principle of mutability has been also established in the properties of life, by which they may be variously altered from their natural state by physical and moral causes; but it is this very principle which enables them to receive salutary impressions from remedial agents, (just as they do from morbific,) and to return to their natural condition.

The changes to which the properties of life are liable, are almost of endless variety; depending upon the nature of the morbific causes, habits, natural and artificial temperaments, age, sex, &c.; and whenever they become diseased, they pass through many progressive changes till they reach the acme of their morbid states. And so, on the other hand, when remedial agents begin their operation, a series of other changes set in, and continue in regular progress until they end in health. The pathological conditions, therefore, of any given disease are constantly varying, and may require frequent variations of treatment; though it commonly happens that the shades of difference are too slight for any essential change of remedies till the pathological changes shall have passed through a considerable series.

This leads me to consider, next, the application of a foregoing principle to the treatment of disease. It being only necessary to establish a peculiar morbid change in diseased conditions that shall favour the operation of the natural tendency of the properties and actions of life to return to their healthy state, a very few remedial agents may be all that are requisite to the attainment of that result; whilst experience shows that our materia medica is encumbered with superfluities. Take a large variety of pathological conditions, such, for example, as are presented by inflammation, it is not necessary that a certain uniform change should be established by the remedies, but only such as shall favour the recuperative tendency. Bloodletting brings about one kind of change, cathartics another, antimony another, mercury another, and so on; whilst each of these agents may prove perfectly curative in many cases of all the modifications to which inflammation is liable from absolute morbific agents. And yet it is obvious that each one produces changes peculiar to itself, whilst the changes induced by either will be as various as the natural modifications of disease. And just so it is in respect to the great variety of remedies which will tend to the cure of intermittent fever. This disease will sometimes yield to almost every thing in the materia medica, and may be suddenly broken up by an emotion of the mind. But every agent exerts changes in the morbid properties of life peculiar to itself, but such changes as enable the properties and actions of life to pass, afterwards, through a succession of spontaneous changes under the restorative principle, till they end in health. There is no other philosophy that will account for any of these phenomena, whilst they all concur in demonstrating its foundation in nature. Hence, also, I may add, what I endeavoured to expound in the "Commentaries," the occasional salutary effects of alcoholic stimulants in the treatment of fever, and acute inflammations, and through which, in part, I attempted to abolish the distinction between active and passive inflammation. (*Com. vol.* 2, *p.* 524—546.) In these examples, the alcoholic stimulants do but introduce morbid conditions that are favourable to the recuperative process, and are, therefore, so far on a par with loss of blood. Nevertheless, a distinction is very properly made into curative and morbific agents, however the former may be productive of disease, as they commonly are, in their medicinal doses when they do not correspond with the existing pathological conditions. Their absolute mode of action, however, is the same in all the cases; and although, in a general sense, remedial agents exert their salutary effects by inducing new pathological states, and are generally liable to produce disease when exhibited in health, these morbid states, when not excessive, are of a nature to allow the full exercise of the recuperative tendency. On the contrary, however, there is a class of agents which are more profoundly morbific, and whose results transcend the natural recuperative process. It is for the removal of these consequences that we employ the other class of morbific agents. Or, there are yet other means, like exercise, air, &c., whose influences are of the mildest alterative nature, and appear to coöperate in a direct manner with a tendency to restoration which had already begun; or, as in whooping cough, where the restorative process is often easily introduced. Our remedies, therefore, are curative by substituting new pathological conditions, and nature does the rest; and it is only with a view to a right interpretation of their *modus medendi* that I have any disposition to depart from established phraseology, or to confound the operation of remedies with that of the ordinary causes of disease.

I now proceed to a statement of the manner in which remedial agents produce their effects upon parts remotely situated from the direct seat of their application; and this, as I have formerly said, is through remote, continuous, or contiguous sympathy; the agents exerting their direct impression upon the parts with which they are in contact. Remote, and, probably also contiguous sympathy, are conducted by the nervous power through the medium of the cerebro-spinal and ganglionic systems; whilst, as I have endeavoured to show in the "Commentaries," continuous sympathy is more or less independent of the nerves. When, however, these enter into the structure of parts, as in animals, they have a certain contingent participation. But their primary connexions may be wholly severed, and disease may be yet propagated continuously along the part to which they appertained; as we observe also in plants. It appears, therefore, that in these examples, the morbid condition is extended, in a continuous manner, from the organic properties of one point to the next in apposition. It is now my purpose to enlarge upon the philosophy of remote and contiguous sympathy, as was represented in the "Commentaries," and in my "Introductory Discourse."

I shall not go into any farther proof relative to the existence of the vital properties as contradistinguished from the properties of inorganic beings, nor of the nervous power. These I assume to be already established, and that they are distinct from each other; and I farther assume that the nervous power is capable of acting as a vital stimulus to the organic properties, is liable to be variously developed by morbific and remedial agents, and to be so modified in its nature according to the virtues of such agents, that it produces, more or less, in diseased parts, remote from the direct seat of the morbific or remedial action, the changes which the agents themselves would exert were they applied directly to the remote organs. (1) The type of all this exists in various processes which are naturally going forward in the animal body, and as profoundly illustrated by the experiments of Sir Charles Bell, Marshall Hall, Wilson Philip, Prochaska, Whytt, Reid, Valentin, and, lastly, as set forth in a masterly manner by Professor Müller in his Elements of Physiology. The nervous power is superadded to animals, for the purposes of voluntary motion, as a regulator of their complex organization, and as an auxiliary to the organic properties; operating as a stimulus to those properties, by which all actions are virtually performed.

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(1) The whole of this doctrine is briefly laid down in the *Medical and Physiological Commentaries*, vol. 1. p. 570—572.

The brain and spinal cord are the great centre of the nervous power, (in the entire state of the animal,) and the nerves of animal and organic life, especially the latter, are the media by which the impressions of morbific or remedial agents are transmitted to the great centre, and through which the nervous power is radiated abroad upon all parts of the organism. The nervous power may be, also, equally determined with a morbific or curative effect upon the organic properties and actions of the great nervous centre; or upon any of its radiating parts. The philosophy is also exactly the same when one diseased part gives rise to disease in parts that are remote; and when disease in remote parts, that has been maintained by affections of other parts, subsides in consequence of the restoration of the latter, it is owing to the removal of a pernicious modification of the nervous power that had been constantly propagated by means of the latter upon the former.

Let us now take a simple physiological example to illustrate our doctrine; and they who have the disposition to pursue the inquiry should study well the laws of sympathy as laid down by Professor Müller, and the elaborate experiments and inductions of Dr. Hall. Such as are skeptical as to the independence of the organic properties of the nervous power, should study Haller, and the great work of Wilson Philip on "The Laws of the Vital Functions." To these works I would also respectfully call the farther attention of some of the present able writers upon the functions of the nervous system, who, as it appears to me, are losing sight of the organic properties, and attributing to the nervous power what belongs to the former.(1) When the

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(1) I do not, of course, concur in the following very important conclusions in Dr. Hall's late "Memoir":—

"The spinal marrow," says Dr. Hall, "exclusive of the cerebrum, is the *source* of animal life."

Animal life, I apprehend, depends as well upon the organic properties, or vital principle, as organic life. The spinal marrow is a "source" of the nervous power, whilst *sensation*, both in animal and organic life, is related to the brain. If, also, sensation or other cerebral phenomena disappear by interrupting the spinal cord, it hardly follows that the brain and its prolongation may not be mutually concerned in all the phenomena which relate to the nervous system, in their state of integrity. Experiments are here surrounded by so many accidents, that their results must be constantly brought to the test of the natural phenomena, or of such experiments as occur in Philip's "Inquiry into foregoing authors are comprehended, the foundation of medical philosophy is laid. But for our physiological example.

> "The whole system of respiratory nerves," says Müller, "can be excited to action by irritation of any part of the mucous membrane, from the mouth to the anus, from the nostrils to the lungs."

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the Laws of the Vital Functions." I distinguish, of course, between *irritability* and *sensibility.* If the former be affected in other parts than the nervous tissue by disease or by injuries of the brain or spinal cord, or by experiments upon them, it is in consequence of a pernicious *nervous influence* which is determined upon that property of organic life. This clearly appears from Dr. Hall's next following propositions, namely,—

> "That the cerebrum is, in its acts of volition, an exhauster of that irritability,—(the irritability of the muscles of organic life:)
>
> "That in muscles separated from their nervous connexion with the brain, we have augmented irritability:
>
> "That in muscles separated from their nervous connexion with the spinal marrow, we have, on the contrary, diminished irritability."

Here the organic property, *irritability,* is variously modified according to the manner in which it is influenced by injuries done to the nervous connexions. The nervous power, also, may be strongly affected by the simple division of a nerve, as we know by the inflammation it may produce in parts which the nerve supplies,—irritability being thus exalted. (*See the Med. and Phys. Commentaries, vol.* 1, *p.* 474—480.) — Or it may be, as I have stated in the "Commentaries," that "when inflammation is excited by dividing a nerve, or by paralysis, the organic forces are directly altered by suddenly withdrawing the nervous influence, as well as by thus deranging the mutual relations between the forces which animate the different tissues of a part; those relations being equally influenced by the nervous power." (*Comm., vol.* 1, *p.* 572.) Again,

> "The irritability of the muscles of organic life," says Dr. Hall, "depends probably on the ganglionic system." (Hall, *on Diseases and Derangements of the Nervous System,* p. 222: 1841.)

On the contrary, I apprehend that *irritability* is everywhere as independent of the nervous system in animals, as it is in vegetables. It is only *influenced* by that system. Nor can it be admitted that "the cerebrum is, in its acts of volition, an exhauster of the irritability of the muscles of organic life." The irritability of those muscles is always in a state of equilibrium in health, as shown by the unvarying nature of the organic functions. When I speak of the brain and spinal cord as the centre of the nervous power, I refer to the animal in its state of integrity. The ganglionic system may be also a centre of this power, and the power may be developed in the spinal cord and in portions of the nerves when separated from their connexion with the brain, or spinal cord. The phenomena of contiguous sympathy are here important, as well as certain experiments upon isolated parts; though we should bear in mind the difficulty of separating, entirely, any part from its connexions with the brain and spinal cord, unless when wholly abstracted from the body.

It is through this principle that emetics and cathartics produce their most sensible manifestations, and the same is concerned in all their influences upon other parts. It is the same as concerns the process of respiration, which takes place after the following manner. The point of departure is the mucous tissue of the lungs, from which an impression (arising probably from the partial absence of atmospheric air) is transmitted through the pneumogastric nerve, as well as through the ganglionic, to the brain and spinal cord, (especially the medulla oblongata,) where the nervous power is developed and reflected upon the organic properties of the muscles of respiration, through the various motor nerves of those organs. These muscles are, in consequence, thrown into action, and the thorax is thus expanded.

Müller says, "like many other persons, I sneeze whenever the bright light of the sun falls on my eyes." (P. 352.) Here the process is more complex, and shadows forth the far more intricate processes that are going forward, — the almost endless circles of sympathy which are taking place, — during the progress or decline of disease, or those which are set up by the operation of an emetic, a cathartic, &c. In the example before us, the sensation is transmitted to the brain and spinal cord, where the nervous power is developed and reflected upon the schniderian membrane. Here a new sensation arises which is sent back to the brain and spinal cord, the nervous power again developed, and, according to relations between that membrane and the respiratory organs, the nervous power is now reflected upon the respiratory muscles, when sneezing follows as the result of the convulsive movement. Again, the mind will do the same thing, when dwelling intensely upon a former paroxysm of sneezing. Sympathetic yawning, and sympathetic micturition, are of this nature; the mind being then the stimulus which develops the nervous power.

The examples of sympathetic influences, through the reflex action of the brain and spinal cord, are almost endless, as they also are in every part of the animal organism. They supply the most ample ground for the interpretation of the effects of remedial and morbific agents in their wide range of influences. The modifications of the circles of sympathy which relate to the respiratory system alone, as in coughing, crying, laughing, yawning, &c., are a fruitful field of inquiry into great and precise laws, and extensively applicable to the philosophy of medicine. The only difference is, that, when disease is established in a part, or when remedial agents operate, the organic properties of the part are altered in their nature, and, of course, the organic actions over which they preside. A specific impression, in the latter cases, is transmitted to the cerebro-spinal axis, and from thence reflected through other nerves upon the organic properties of other parts, and, according to its nature, disease will be produced or mitigated in those parts. However complex and variable, therefore, the phenomena, nothing can be more simple than the principle through which all these changes are produced.

When an emetic operates, the philosophy of its influences is the same as that which relates to respiration, &c. The impression upon the stomach is transmitted to the brain and spinal cord through the pneumogastric and ganglionic nerves, the nervous power developed and reflected in the foregoing manner upon the respiratory nerves, whilst another current descends along other fibres of the pneumogastric to the muscular tissue of the stomach. If the emetic operate, also, as a cathartic, then a new chain of actions is established, in the same way, in the abdominal muscles, whilst a current of the nervous power is propagated upon the muscular coat of the intestines. (1)

(1) Among the sound practical conclusions, and shrewd observations, for which Dr. Holland's *Medical Notes and Reflections* are distinguished, and which reach far into the profound philosophy of life, is the following remark. "A single emetic," he says, "may cut short a paroxysm of asthma for which opiates and antispasmodics have been employed in vain." Dr. Holland has also propounded the essential mode in which the emetic produces relief; and were the same philosophical principles more generally understood, we should less frequently witness abortive attempts to accomplish by "opiates and anti-spasmodics" what may be often "cut short" by an emetic, or again by loss of blood. Thus:—

"I need hardly advert but slightly," says this philosopher, "to the benefit derived from emetics in asthma; though here again it must be admitted that there is an insufficient use of the remedy, seeing the great good gained in many such cases by unloading the stomach and liver; and the *equal advantage*, though less obvious in explanation, from its influence on the actions of the circulation and nervous system." (P. 313.)

A part of the "influence" through the medium of the nervous system, I apprehend to be the following. During the paroxysm, the respiratory muscles are employed in embarrassed acts of respiration; but, as soon as the emetic operates, they are thrown by the nervous power into a new series of convulsive movements, by which the respiratory are brought back to their natural condition. If the stomach be offended by irritating food, &c., its removal alone may take off the main cause of that peculiar development and determination of the nervous power which affects the respiratory

But, in the foregoing case something more happens than in the natural processes. Here the exciting cause possesses peculiar virtues, is of a morbific nature, and it not only makes peculiar impressions upon the alimentary mucous tissue, according to the exact nature of its virtues, but it modifies the nervous power in a corresponding manner. If the stomach be the seat of disease, the direct impression upon that organ, or the change which an emetic may effect in its vital condition, will be more or less varied from what is exerted in a state of health. It may, therefore, prove curative to the stomach by this direct influence. But the nervous power is also modified according to the impression produced upon the organic properties of the stomach, and is sent abroad, with alterative effect, upon various parts of the system. According to a law by which diseased parts are far more susceptible of influences from vital stimuli than such as are not diseased, the modified nervous power will fall with far greater effect upon the former than the latter. The organic properties and actions of one may be profoundly and permanently affected, whilst the latter are only moderately and very temporarily influenced. In consequence, also, of the deep effect which the modified nervous power exerts on the diseased parts, they may return, at once, to their natural state.

But the milder influences which are set up by the nervous power upon parts in health, or in comparative exemption from disease, play, also, their part in the salutary process. If the emetic operate also as a cathartic, impressions are transmitted from the intestinal mucous membrane to the cerebro-spinal system, the nervous power developed and modified according to the nature of these impressions, and radiated abroad as when the result of the action of the emetic upon the stomach, and with effects corresponding to this new development and modification of the nervous power.

Again the skin is influenced in the foregoing manner, and this organ transmits that impression to the cerebro-spinal axis, and develops and modifies the nervous power accordingly, when it is, as in the other instance, reflected abroad, and is felt by various parts according to their degrees of susceptibility. Various

muscles, and thus, mostly, the paroxysm may be removed. In other cases, where bloodletting is the remedy, there is a state of venous congestion of the lungs, — perhaps, also, of the liver. The loss of blood then relieves by subduing the morbid state of the veins. (*See Medical and Physiological Commentaries, vol. 2, p. 484--502, where this subject is fully investigated.*)

other circles of sympathy of the same nature set in, and become too complex for analysis; but all may fall with one concurring, curative effect upon the diseased susceptible organs.

We thus see that when vomiting springs from the operation of tartarized antimony, and often from ipecacuanha, it is only one of the consequences, and a minor one, of the peculiar irritation of the gastro-mucous membrane. Other and far more powerful influences are determined, simultaneously, upon the organic properties and actions of distant and diseased parts, (perhaps as distant as the most remote extremity,) by the same nervous power that shook the respiratory organs during the act of vomiting. And often, indeed, does it happen that those influences are propagated with the most profound effect, when the act of vomiting fails of being consummated; and nausea alone shall send with prostrating effect the modified nervous power over the whole system; when we shall see it simultaneously bathing the whole surface with perspiration; pouring the saliva from the mouth; breaking down a tumultuous excitement of the heart and arteries; starting on the instant a torrent of bile, and an equal effusion from the intestinal mucous membrane; and, at the next moment, calling up a magnificent play of sympathies for the evacuation of the fluids, after the manner of an active purgative, — these very effusions, also, instituting other circles of sympathy, which join in the great work of curative movements. Should vomiting now follow, then shall you speedily see the vital energies returning, — the cold pale skin giving place to a florid hue and a warm perspiration, — the sunken features starting into the fulness of health, — the gastric suffering gone as a luxury obtained, — the general whirl of anxiety and distress converted into calm tranquillity, — the headache dissipated, — the twang of the croup, or the grunt of pneumonia, no longer sounding an alarm; — and, all this stupendous succession of events, from the beginning of nausea to the restoration of the vital energies and the near resolution of disease, — composing a most astonishing consecutive series of sympathies, — may require less time than I have hastily employed in this general allusion to the subject. And now can it be entertained, that this has been the result of absorption, or that the laws of chemistry or physics have had any connexion with the phenomena?

The foregoing may be taken as an example of the principle which concerns the *modus operandi* of all curative or morbific agents, whether physical or moral, and of all the developments of disease that arise as sympathetic consequences of each other. In respect to emetics, however, it should be considered that all do not produce the foregoing effects, and that with the exception of the act of vomiting, the results will depend upon the precise nature of the emetic, or the manner in which it modifies the nervous power and thus impresses the organic properties. This explains the difference in results between tartarized antimony, ipecacuanha, sulphate of zinc, warm water, tickling the fauces, the mechanical irritation of undigested food, the shock of a fall, of a surgical operation, sailing, whirling, offensive sights, offensive odours, loss of blood, and even their recollection; whilst the nature and effect of the greater number should lead the philosophical inquirer to pause at the physical doctrine of absorption, and survey the other difficulties with which it is fatally encumbered.

When the alterations, of a sympathetic nature, are more slowly produced, as when mercury gradually induces salivation, and brings the whole system under its influence, or when small, and repeated doses of tartarized antimony overcome inflammations of the lungs, &c., the nervous power is developed and modified at each successive dose, and the repetition of its influence upon the organic properties of diseased parts remote from the stomach establishes progressive changes, till an absolute condition of disease may be induced in certain parts, as when mercury salivates; whilst the analogous influences which are exerted on parts already diseased supplant the naturally morbid states by others of an artificial nature, from which the organic properties are able to return to their healthy condition. But these impressions must be frequently repeated; for if the interval be long between the administration of the doses of such agents as only produce their effects in a gradual manner, the diseased conditions, not being placed in the way of the recuperative tendency, will throw off the artificial impression, and the original intensity of disease will be thus restored. The process which I am now considering is an example of the *cumulative* effect of remedial agents, some of which are much more remarkable than others, and the ultimate results are pronounced with varying degrees of suddenness. This is also influenced by peculiarities of constitution, or of susceptibilities of the organic properties to changes now under consideration; and therefore is it, that salivation may be speedily induced in one subject by less than a grain of calomel, whilst no amount of the remedy will produce this effect in others. And so of the morbific effects of digitalis; an agent, also, which exemplifies the instantaneousness with which alteratives may produce an explosion of disease, although no symptoms had admonished us of its approach. This principle concerns, also, the predisposition to disease which is formed by miasmata, the virus of smallpox, of hydrophobia, &c.

The permanent operation of the nervous power in particular parts of the animal fabric, as in the sphincters, supply an elegant parallel with the foregoing uninterrupted influences of the same power as developed by remedial or morbific agents. This power operates as a perpetual stimulus to the organic properties of the muscles just mentioned, in the same way as blood does to the heart and capillary arteries. And now, if we mutilate the inferior part of the spinal cord, or observe the sphincter ani when relaxed in bad cases of apoplexy, or regard its condition when the spinal cord is merely divided, we shall see the relative bearing upon other organs of these two parts of the nervous system in their connected state, but with injury of the brain, and how the spinal cord is capable of an independent influence. (*See note at page* 42.)

When moral causes operate in the cure, or production of disease, they act directly upon the cerebro-spinal axis, and develope and modify the nervous power according to the nature of each mental affection; and, as in the case of physical agents, the nervous power thus developed and modified may be determined as well upon the organic properties of the brain and spinal cord, as upon other parts. (*See note, page* 42.) The blow upon the region of the stomach, or the opening of a thecal abscess, which have destroyed life on the instant, operate in the same way as the paroxysms of anger, or of joy, which have been as suddenly fatal. In these cases the nervous power is first determined with a fatal effect upon the organic properties of the nervous centre.

The artificial modifications of the nervous power have analogies in the natural modifications of sensibility, irritability, &c., in different organs and tissues, and in tissues of the same nature; as the mucous for example. But, there are more striking analogies in the artificial temperaments, and as the organic properties are affected in the various conditions of disease.

We need not inquire into the nature of the nervous power, or of the organic properties. It would be as absurd as to interrogate the nature of gravitation, or of any other property of mere matter, or even of matter itself; though we may well say what the nervous and organic powers *are not*, and thus save much speculation and its resulting practice. We know enough of the whole, for all the purposes of philosophy, and for the good of mankind, by the phenomena alone; but since the phenomena of organic beings are far more diversified than those which relate to inorganic matter, so, also, should we be as contented with the former as with the latter, and apply them in the same philosophical and practical manner. We also know enough of physics to marvel at nothing in organic beings which may be utterly different from the constitution, the phenomena, and the laws of inorganic matter; and if it seem mysterious that such an agent should exist as the nervous power, with the characteristics which I have here assigned, it will become less wonderful when we reflect upon the phenomena of the immaterial mind in its connexion with organization, as in voluntary motion, blushing, palpitation, syncope, apoplexy, &c., or even upon the velocity of light and the inconceivable rapidity of its undulations; and, above all, that it is not an improbable doctrine, that the light, heat, and deoxydizing property, of the solar beam, reside in a common principle, and are due to modifications of that principle by foreign influences. So, also, in respect to the modified developments of the common agent upon which the phenomena of electricity and galvanism depend.

Having now completed the outline of the doctrines which it was my purpose to set forth in a more connected manner than on former occasions, I shall add a few explanatory remarks.

The impressions which are made in organic life, by vital agents, whether natural, morbific, or remedial, are mostly upon the *irritability*, but partially on *sensibility*. (*See note*, *p.* 42.) The latter property is the subject of impressions particularly in animal life; though it becomes more or less involved in organic by the accidents of disease. (*See Med. and Phys. Commentaries*, vol. 1, p. 471—480, p. 568—572; vol. 2, p. 18—20.) *Perception* is necessary to *sensation*, and whenever actions take place independently of sensation through the medium of the nervous power, the impressions are transmitted to the cerebro-spinal axis by the *irritability* of the nerves. In organic life, the reflected nervous power operates, first, upon *irritability*, from which it is imparted to *mobility*, &c., and this is probably also true in animal life; though in the latter case, when sensation arises, the impressions which are transmitted to the brain and spinal cord are through the medium of *sensibility*. In voluntary motion, the mind develops the nervous power and directs it upon the *irritability* of the particular muscles. In all the cases of primary impression, however, where *perception* is excited, *sensibility* is more or less interested. When morbific or remedial agents operate, they modify the *irritability* of the part upon which their direct action is exerted, and in a way peculiar to the virtues of each; which is the cause of the modifications of the nervous power. These, being reflected upon the irritability of diseased parts, modify that, and consecutively the other organic properties, according to the acquired peculiarities of the whole. The principle is the same as it respects the passions and all the mental emotions. Our remedial agents, therefore, will be curative or morbific according to the alterations which they may thus produce. I cannot enter upon the details of facts by which all this is perpetually illustrated. Every mind is adequate to the inquiry. If the brain, or any part of the nervous system, be the seat of disease, of irritation, &c., the preternatural development of the nervous power is *direct*, and exceedingly various in its nature and intensity. It is also liable to remarkable developments and results from the mechanical irritation of remote parts; as when undigested food produces apoplexy, or a blow upon the epigastric region, or a slight surgical operation, is followed by instant death.

The nervous power is not only variously excited, exalted, or depressed, or modified in its kind, and produces influences upon remote parts according to these changes, but it is reflected upon particular parts according to their existing susceptibilities, the nature of the remote cause, and the part upon which the remote cause may operate. Thus one impression from cold, as a blast of cold air, or a drop of cold water upon the skin, will rouse the respiratory muscles. Another impression from the same cause will excite catarrh, or pneumonia, or articular rheumatism. (*See Med. and Phys. Comm. vol.* 2, *pp.* 18, 41—50.) Mercurial ointment will determine the nervous power specially upon the salivary glands, and liver, and the same effects arise from the action of mercury upon the stomach. Canthandes, internally or externally applied, irritates the neck of the bladder. One degree of impression by tartarized antimony upon the stomach determines the nervous power upon the respiratory muscles, and vomiting is the consequence; whilst it simultaneously reflects the same power upon the skin, as it does in smaller doses, and of which perspiration is a consequence, — and so on. But these examples embrace only certain parts of the influences in each case; whilst in others, they are far more complex, — one sympathetic result becoming the cause of others, till, through a single impression upon the organic properties of the skin, various circles of morbific or remedial sympathies may be instituted. Narcotics induce peculiar modifications of the nervous power when they are administered by the stomach, and the power thus modified is not only reflected upon various distant parts with effects corresponding with its modifications, but especially, also, upon the organic and animal properties of the brain and spinal cord. Hence the obtuseness of the senses, and the venous congestions of the brain, which follow their improper administration. (*See Med. and Phys. Commentaries, vol.* 2, *p.* 480—488.)

Notwithstanding, however, the complexity of the results that arise from the sympathetic processes in which the nervous system is concerned, whether excited by healthy, morbific, or remedial agents, they all depend on a common law.

I have but little to add specifically upon the doctrine of the absorption of remedial and morbific substances to what I have extensively set forth in my treatise on the Humoral Pathology. I will, however, state a few objections more, which, like those offered in the Commentaries, will suggest a multitude of others. I may point to one, also, which I have stated in my Materia Medica, (p. 180,) relative to the variety of results which follow different quantities of certain preparations of iron, whilst it cannot be said that the slight natural ratio belonging to the blood is increased. The same considerations are equally applicable to a vast number of remedial agents. Wedemeyer and Müller testify to the fatal effect of one drop of the hydrocyanic acid within a single second, when introduced into the eye of a rabbit; whilst Müller allows that from half a minute to two minutes are necessary to the absorption of other substances. Magendie kills "the most vigorous dogs" by applying to the fauces one drop of the hydrocyanic acid, "after two or three hurried inspirations." Pereira says that he "once caused the *instantaneous* death of a rabbit by applying its nose to a receiver filled with the vapour of the pure acid. The animal was killed without the least struggle." And so did Magendie. Pereira adds, that in cases of this nature, "the rapid action of the poisons seems almost incompatible with the idea of their absorption." (*Pereira's Materia Medica, pp.* 27, 242.) Look at the action of opium. Apply it to the mucous tissue of the intestine, the local impression is such that it immediately arrests the peristaltic movements. (*See Med. and Phys. Commentaries, vol.* 1, *pp.* 477—480, 506—512, 563—576, *for many analogous experiments, and for an examination of Prof. Müller's doctrine that "the narcotic action of opium does not react from a particular point of a nerve on the brain*.") Apply it to the surface of the brain, and it instantly lessens the action of the heart, and capillary blood vessels, &c. Now combine these phenomena, when opium exerts its direct action upon the stomach, and indirectly upon the heart, capillary system, &c., and consider the natural relations between the stomach and nervous centre. Take a substantial, *physical* fact, as supplied by the advocates of absorption. Thus: —

> "It is *very singular*," says Sigmond, "that a pill of opium, administered at night, will be VOMITED UP in the morning, *after having produced its narcotic effect*. This is an observation which Van Sweiten originally made." SIGMOND'S *Lectures, in London Lancet*, 1836-7, *p.* 423.

The phenomenon, however, ceases to be so "*very singular*" the moment my doctrine is admitted.

> "I am acquainted with a physician in London," says Sigmond," "who, on taking opium, although in a very minute quantity, will have over the surface of the body a scarlet efflorescence." — *Ibid.*

Is not this phenomenon due to the same principle as that which is concerned when indigestible food occasions analogous eruptions, or when they spring up, as in infancy, especially, from gastric and intestinal derangements?

The snuff which regales the nose, and the tobacco which equally delights the mouth, are violent poisons to the intestinal mucous membrane; and the constitutional results harmonize with the local effects in either case. Again, if remedial, or poisonous substances act by absorption, why is tobacco smoke so innoxious when inhaled by the lungs, and yet so deleterious when swallowed, or when conveyed into the rectum? Most remedial agents, indeed, produce constitutional effects according to the natural vital modifications not only of the mucous, and other tissues of different parts, but of one continuous tissue, as the mucous membrane of the eyes, nose, fauces, œsophagus, stomach, small and large intestines, larynx, trachea, and lungs. Where would philosophy be; where our interpretation of these various consequences, if we followed the chemist in his physical views of life? What would tobacco affect in such a case? Would it nauseate by affecting chemical affinity, or cohesion, or elasticity, or would the nose or the mouth enjoy through any such properties of matter, — or would galvanism help our understanding? Is it through any such properties that we feel the smart when the fire burns? Does not Pereira supply an important fact against his general doctrine of operation by absorption when he defends a moderate practice of opium smoking, — especially as the whole volume of smoke is drawn into the lungs? (*Mat. Med. p.* 1293.) Shall we not rather look to what is known of the natural modifications of irritability in the mucous tissue of different organs? If opium offend the stomach, the principle is the same as when urine excoriates the membrane of the lungs, and thus produces the most violent constitutional effects. But the distinguished author above quoted shall lay down our principle himself. Thus: —

> "Sir B. Brodie," he says, "found that an infusion of tobacco, thrown into the rectum, paralyzed the heart, and caused death in a few minutes. But if the head of the animal be previously removed, and artificial respiration kept up, the heart remains unaffected; proving that tobacco disorders this organ through the medium of the nervous system only." (*Ibid., p.* 869. *See, also, my note, p.* 42.)

Should we not rather say, through the medium of the brain in its connexion with the spinal marrow, whilst other parts may be sympathetically affected through the spinal marrow or even the ganglionic system, alone. (*See Med. and Phys. Comm. vol.* 1, *p.* 570 — 572.) And now contrast with the foregoing peculiarities of tobacco and opium, the fact that the inhalation of the fumes of hyosciamus produces vertigo, tremors, laborious respiration, &c.; and that hydrocyanic acid, in the quantity of a drop, or in vapour, on account of the coincident relations of its virtues to the naturally modified organic properties of various parts, is instantly fatal, whether applied to the mucous tissue of the eyes, nose, mouth, stomach, or lungs. And so of the spirituous extract of nux vomica. If absorption be good in some of the cases, it should be equally so in the others. (*See extract from Bichat, p.* 36.) Consider, too, how the habitual use of narcotics reduces the susceptibility of the stomach to the influence of each one, respectively, and not to the others, and how the constitutional effects go on, *pari passu*, in the ratio of the local effects. And consider, also, how music assuages suffering, or the expectation of the dentist relieves the tooth-ache. And why, according to the doctrine of absorption, should not medicines produce the same constitutional effects when injected into the bladder, as when administered by the stomach? Are you doubtful as to the manner in which certain substances produce their constitutional effects, when applied to the skin, as mercury and tobacco, for example? Consider the foregoing case of hydrocyanic acid; or how an issue relieves deep seated inflammation; or, again, how belladonna, or hyosciamus, when applied to the lids of the eyes, as when to the stomach, produces dilatation of the pupils.

Again, let us observe the constitutional effects of tartarized antimony, when administered in small and repeated doses. This substance possesses, in a general sense, the power of lessening the irritability of the stomach, (in relation to its own virtues,) where the doses are small at first, and gradually increased. From this principle, indeed, results the necessity of increasing the doses as far as they may be borne without nausea, for the purpose of maintaining the same influence upon disease as is exerted by the first and smaller doses. In this way, in certain affections, as in croup and rheumatism, we may sometimes rapidly increase the doses from the sixteenth of a grain to two grains, although the first dose shall have actually produced vomiting, whilst the two grains are borne without nausea. It is also certain that this progressive increase of the remedy, as far as may be admitted by the stomach, is indispensable to the full influence upon disease which was exerted by the smaller doses before the remedy had subdued the irritability of the stomach.

Now were the physical, and not the physiological, doctrine true, there should be no necessity for this regular and rapid increase of the doses. The nearer, indeed, each dose approaches the point of nausea, so will the general arterial excitement, and local inflammations, be held in subjection; from which it is plainly manifest that the remote effects depend upon the amount of influence produced upon the stomach. And so of opium, and all the narcotics, and, indeed, of various other agents which are freely assumed to operate through the circulation.

But again on the contrary, we may obtain an exactly opposite series of results from tartarized antimony; by which we prove our proposition by the converse of the foregoing phenomena. We may begin the treatment by one-eighth of a grain without producing nausea; but in an hour or two afterwards, a repetition of the same dose nauseates the stomach, and prostrates the whole system. Again, at the same interval, we repeat the same dose and vomiting ensues, accompanied by still greater constitutional effects. We then reduce the quantity to the twelfth of a grain, and again we have nausea and vomiting, with still greater constitutional results. We go on to reduce the dose in this manner, and, as I have witnessed in adults, it has been necessary to diminish the quantity to the thirtieth part of a grain to avoid protracted nausea, and a general prostration of the system. Here, then, the remedy not only continues to nauseate the stomach in greatly diminished doses, but, as in the opposite case, there is a constant ratio between its impression on the irritability of the stomach and its constitutional influences and its special effects on diseased remote organs. However the dose may be diminished, so long as it impresses the irritability of the stomach, it breaks down the general arterial excitement, and often overthrows inflammation just as fully, and rapidly, as when two grains are administered with a similar effect upon the stomach. Nor is this all which antimony opposes to the doctrine of absorption; since in the cases first supposed, when it finally produces nausea after repeated and gradually increased doses, it does not reduce the irritability of the stomach after that dose, as after the beginning of the remedy, and when it did not produce nausea. On the contrary, the gastric irritability is now brought up to a full relation to the remedy in that last dose, where it either remains permanently for some time, or is quite as apt to increase in susceptibility to the antimonial influence, so that it may be necessary to diminish the next following dose to avoid a renewal of the nausea, and perhaps vomiting. In the meantime the effects on the constitution, and on remote disease, are exactly conformable to the amount of influence upon the stomach.

Pereira has rendered his valuable work on Materia Medica liable to the objection which I am now considering, as he has, also, to that of reasoning from the effects of remedies on man in health, and even upon the naturally modified constitution of animals and plants, to the altered susceptibilities of man as they exist in disease. (1) Of tartar emetic, he says, we do not know "the mode in which it produces its curative effects." And again,

> "Shall we deny the efficacy of bloodletting in inflammation, of mercury in syphilis, of cinchona in intermittents, and of a host of other remedies, simply because we *cannot* account for their beneficial effects? The fact is," he continues, "that in the present state of our knowledge, we cannot explain the *modus medendi* of a large number of our best and most certain remedial agents." (*Pereira's Mat. Med. vol.* 1, *p.* 417. 1839.)

This supposed ignorance is mostly predicated of the failure of detecting the medicines in the circulation; but will it apply to such observers as explain their *modus operandi* on other principles, and in conformity with well established facts? If "bloodletting be efficacious in inflammation, mercury in syphilis," &c., they are so through great and immutable laws; and shall we rest in ignorance of those laws because we cannot deny the efficacy of the remedies? Is it not this very common representation of the topics before us, and of the phenomena of living beings, which has led to so general a disregard of the great principles in medicine, and to the revival of the exploded creeds of the iatro-chemical and iatro-mechanical philosophers? Or is it any argument against the interpretation of the properties and laws of organic beings, of their modifications in disease, of the *modus operandi* of remedial agents, as set forth by one inquirer, that fifty different and conflicting systems have been projected by others? Such, indeed, must be the position of every disputed topic when universal truth shall ultimately prevail. The argument, therefore, however common, is necessarily fallacious.

There is no objection to admitting that all remedial and morbific agents find their way, very scantily, into the circulation, excepting as it regards the matter of fact, and a respect for those principles which nature has ordained for their exclusion so far as to prevent their ingress in injurious quantities. (*See this question largely examined in my Essay on the Humoral Pathology.*) No conclusions, as I have shown, can be formed from the effects of injections into the circulation; which are the rudest facts in relation to a topic of this nature. It therefore becomes the merest assumption to affirm that the minute proportions of medicines, which may steal their way through the well guarded portals of the organism, produce those remarkable results which we witness after their administration by the stomach; whilst we are met at the threshold of the inquiry by the clearest interpretation of their *modus operandi* in the perfectly demonstrable laws of sympathy, in a stupendous display of the operations of the nervous power in the natural conditions of the body, and as modified by a vast variety of experiments, and by the morbid processes that are perpetually before us. I have formerly endeavoured to indicate how derogatory to medical philosophy is the doctrine of operation by absorption,—how pernicious its practical tendencies.

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(1) Our able author, however, sometimes sustains the physiological doctrines. Thus, of Elaterium, he says that, "in cerebral affections, such as apoplexy, mania, &c., elaterium, as a drastic purgative, sometimes proves serviceable on the principle of counter-irritation." (*Ibid.* p. 1088.)

Again, take the grand characteristics of the cinchonas, arsenic, calomel, and the whole group of agents for intermittent diseases. Of cinchona, Pereira says, (after having expounded its operation as a tonic through the process of absorption,) that in intermittent diseases its "*methodus medendi* is quite inexplicable." (*Ibid. vol.* 2, pp. 1002, 1006. 1840.) But, is not its mode of operation just as intelligible in one case as in the other? Does not the whole system of nature, where common results are concerned in any integral part, enforce the belief that the same laws are concerned in both cases; and do not all the relative facts in physiology, all that is known of the properties of life, and of the constitutional effects of vital stimuli of any denomination, proclaim the fact, and that nature is just as consistent in this instance, as she is in the simple principles which determine the phenomena of gravitation, of chemical affinity, of the attraction of cohesion, of repulsion, &c., or, in more sensible physics, of electricity, of light, of magnetism, &c.? If we refer, as does Pereira, to the effects of cinchona as a tonic, upon the healthy system, we must explain the *methodus operandi* before we can apply it in the least to any parallel effects upon morbid and enfeebled states of the system. But we may not speak of "augmentation of cohesion of the organic mass," &c. (*Ibid.* p. 1002.) These are only effects of an antecedent operation, in which the whole *modus operandi* consists. But the mode in which cinchona produces its effects in the perfect organism being just as obscure as in diseased states, we start with our interpretation of its *modus operandi* in intermittents, just as we do of the mode in which cinchona produces its fullest effects in health; or raises the vigour of the stomach, sharpens the appetite, and braces up the animal man, in dyspeptic affections.

Now the mode in which cinchona accomplishes these last results is no more obvious than its action as a febrifuge. One must certainly be as plain as the other, since the essential influences and changes are exerted upon the organic properties of living parts, which are governed by simple and immutable laws. To explain the operation of a given cause upon two principles where the results are of the same genus, and nearly of the same species, would be to disjoint nature completely, and to render her a deformity.

With this fundamental principle, we move forward to the interpretation of the effects of cinchona when it exasperates or produces disease; and so of other morbific agents. All the results, as they vary from those which follow the ordinary stimuli of life, depend upon the mutability of the organic properties and actions. Upon these, morbific causes, like the natural vital stimuli, make their whole impression; but they go farther in that impression than the natural stimuli of life. That is to say, they make their impression so profoundly, and in virtue of their peculiar attributes, as to alter the natural condition of the organic properties and actions; and this alteration constitutes disease. All that follows are but mere "sequelæ." Remedial agents, as we have seen, are capable of doing the same thing; and when direct in action, they operate upon the same principle. It is for this reason, therefore, that they produce disease in the healthy organism; and when they contribute to the cure of disease, it is in virtue of that morbific action which they exert on healthy parts. They are a class of morbific agents, however, which produce only such diseases, in health, (if not administered in great excess,) as are of a transient nature; and when, therefore, administered for the cure of disease, they induce a morbid state more favourable than the pre-existing to the natural tendency of morbid organic properties and actions to return to their healthy standard.

Thus we get at a common principle of the *methodus operandi* of cinchona as a *tonic*, as a *febrifuge*, and as a morbific agent; and it is equally applicable to all remedies which possess absolute remedial virtues. This philosophy enables us at once to understand how arsenic, cobweb, opium, alcohol, moral emotions, and almost every thing else, are, like cinchona, more or less curative of intermittent fevers; and though the alterations which are directly instituted by these various agents are unlike in all the instances, and correspond with the peculiar virtues of each agent, each one induces such changes in the organic properties as enable them to take on their natural tendency towards a state of health, — some being more conducive than others, and either liable to exasperate the disease. We thus see, also, how it is that our remedies must be well adapted to the existing pathology, or they will prove morbific; since their operation is as well regulated by the nature of the morbid conditions as by the virtues of the remedies.

That there *is* a *methodus operandi*, in all the foregoing cases, is too certain to be questioned; and such being the fact, it is quite a becoming occupation for the human mind to interrogate its nature; or as Bacon has it, "it is the Glory of God to conceal a thing, and the glory of man to find it out."

I will now present a comprehensive example which illustrates the foregoing doctrines. A seton, passed through the skin of the neck, or even of the arm, removes inflammation of the eyes. In this instance, nothing can possibly enter the circulation, but the whole influence of the seton upon the eyes must be exerted through the medium of the brain and spinal cord. By tracing out all the effects of which this seton is capable, we may show that it involves all the principles which are concerned in the production of disease and its cure.

In the first place, the seton establishes an inflammation in the part of the skin in which it is inserted. Here we have the whole interpretation of morbific agents in producing their primary diseases. Like the seton, all others act upon the irritability of parts, to which they are applied, alter its nature, and involve the other organic properties in corresponding changes, when a change of function ensues as a consequence; and then may follow a variety of physical results.

Now let us consider the seton in its curative aspect, as it relates to the ophthalmic inflammation. The morbid state of the skin operates as a peculiar stimulus, the result of which is transmitted to the cerebro-spinal axis, where it develops and modifies the nervous power, which is then reflected upon various parts. But the intensity of the nervous power, thus developed, is not sufficient to alter the organic properties of any part excepting the susceptible ones which conduct the inflammatory affection of the eyes through their instruments of action, and therefore no sympathetic disease is produced. But irritability being preternaturally susceptible in the inflamed eyes, the nervous power operates with effect upon it, and alters the nature of that and other properties so as to enable them to return to their natural state; and thus the inflammation subsides.

We will next see how this seton may become the cause of sympathetic diseases in remote parts, and we shall then, also, have the whole of the principle which is ever concerned in the development of secondary diseases; and we shall see, too, that the principle is precisely the same as that which concerns the curative effects of remedies when they operate upon remote parts through the medium of another part; as in the curative effect of the seton upon the inflamed eyes.

Let us then suppose that the seton is permitted to remain in the neck after it has accomplished the cure of the eyes, till, finally, it excites a severe degree of inflammation in the surrounding skin. By-and-bye, we find the patient beginning to lose his appetite, the tongue coats up, and other marks of a diseased state of the stomach set in. This organ, therefore, has become involved in disease in consequence of the neglected and irritative state of the seton. Still, however, the mischief is allowed to go on, and the eyes, which had been relieved by the seton, again become inflamed. The seton has been the essential cause of this round of phenomena; and since nothing can have been introduced into the circulation, from beginning to end, we must look to the nervous influence for the remote developments of disease, as in the former case, for the curative results. The seton, after the cure of the eyes, had taken on a higher and modified state of inflammatory action, and it transmitted to the brain and spinal cord such influences as developed the nervous power in greater intensity and a more morbific condition. This state of the nervous power, being reflected abroad, fell with greater force upon the stomach than on other parts, from its peculiar susceptibilities, and its close natural relations with the skin and cerebral system. The stomach has also the eyes much under its control, and the eyes are now particularly liable to be injuriously affected by sympathies propagated from the stomach on account of their recent inflammation, which left them in a more than usually susceptible state. The stomach, therefore, in transmitting its morbid impressions to the cerebro-spinal axis, cöoperates with those from the seton in increasing the nervous influence; which, being determined with a morbific effect upon the eyes, produces the ophthalmic inflammation. (*See example of hydrophobic virus, in Med. and Phys. Commentaries; vol.* 1, *p.* 499.)

We have now to consider the natural tendency of the properties and actions of life to return from diseased to their healthy states. The seton, as we have seen, is the sole cause of the new developments of disease in the stomach and eyes, and these effects are maintained by keeping up the irritative inflammation of the skin. If, therefore, we withdraw the mechanical irritant from the skin, the inflammation of the part will subside spontaneously; and having thus removed the exciting cause of disease in the stomach and eyes, these parts, also, return spontaneously to their healthy states. Thus it is, also, that the irritation of setons, issues, blisters, &c., when applied over the joints, &c., for the removal of inflammation of the ligaments or other tissues, may, after having greatly fulfilled their purpose, ultimately keep up a degree of the disease, or increase its intensity. But, if the skin be now healed, the disease will subside spontaneously,—the very healing of the skin reflecting salutary influences. This recuperative law lies at the foundation of therapeutics, and it shows us that the first and greatest step in the treatment of diseases is to remove their exciting causes; when nature may require no other assistance from art.

The only remaining consideration to complete the essential philosophy of the operation of remedial and morbific agents, relates to the direct action of remedies in curing diseases of parts to which they may be applied. If an emollient poultice, as it is called, or opium, or leeches, &c., be applied to the inflamed skin, they may hasten the subsidence of the inflammation. This is done by their direct alterative action upon the diseased properties of the part, as in the case of morbific agents; and in proportion to the subsidence of the primary affection may be that of the sympathetic diseases. But, the sympathetic affections may be also hastened in their decline by the direct application of remedies to the sympathising parts; or, we may contribute to the cure of the whole by addressing remedies directly to one of the organs which has been sympathetically involved, as to the stomach in the foregoing case; or, the sympathetic affections may go on independently of the cure of the primary disease, and require a distinct treatment; or, it may be necessary to cure them first before the primary disease can be removed. The diseased state of the stomach, for example, in the foregoing case, may, in its turn, establish a morbid sympathetic influence over the seton, and thus complicate the principle as to exciting causes, and institute a mixed condition of sympathetic influences. This, in fact, is more or less the case, in most diseases, after the morbid state is propagated from the primary seat. In the example now stated, all the diseased parts act and react upon each other, each becoming a point of departure for the development of a morbific nervous influence, and each affection, therefore, contributing to maintain and aggravate the others. Other organs join in, though perhaps not essentially disturbed, and take their part in the disease, according to their degrees of affection, and more or less, also, according to their relative vital importance and constitutional relations; whilst the great movement of diseased action may be variously influenced by the contingencies which grow out of constitution, temperament, age, habits, external influences, &c.

The affection which has been celebrated under the name of *spinal irritation* is virtually, in most cases, that disease of the stomach which others have denominated *indigestion.* Where the spinal cord is the seat of irritation, the principle is involved which I have just stated,—forming an example of sympathetic affection of the nervous tissue. In most of the cases, the primary and important seat of disease is in the stomach, whilst the remedies are apt to be abortively applied to the spinal cord. The blow, on the contrary, should be struck at the gastric affection upon which the sympathies depend; and, in a large proportion of the cases, but little else is necessary than a rigorous attention to food, exercise in the open air, and regular habits.

The state of the spinal cord is not worth a local remedy in most of the cases; but, when the pain and soreness of the cord is great, an injurious nervous power may be propagated back to the stomach, and more or less relief will then be contributed by the application of leeches and blisters along the spine; or, in fewer instances, the spinal irritation amounts to a degree of disease that must be cured before the original gastric affection will surrender.(1) The "clergyman's sore throat" is another example of this nature, and where the remedies should relate to the diseased abdominal viscera.

If what I have now, and hitherto, laid before the profession, be founded in nature, it is obvious that all of us, who would attain a proficiency in the science of medicine, must carry out the principle which is inculcated by the injunction, — "*nocturna versate manu versate diurna.*"

"I have not forgotten a remark," says my friend and late distinguished President of the Liverpool Medical Society, "made by a venerable professor in Edinburgh, (the late Dr. Duncan, sen.,) in one of his introductory lectures, *that a medical man must make up his mind to be a student all his life*; and I can with truth aver, that every year, nay, every day, adds farther experience to fully confirm in my mind the truth and accuracy of the remark." — DR. THOMAS JEFFREY'S *Address to the Liverpool Medical Society*, 1835.

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(1) Dr. Holland has some luminous remarks upon this subject, which it will be well for those to examine who fancy that the seat of the greatest disease is denoted by the greatest pain. His statement, that in many cases, "recovery is derived, not from remedies applied to the spine, but from steel, bark, ammonia, and valerian, cold water bathing or washing, exercise, moral remedies, &c." goes to confirm the suggestions which I have made in the text. — *See* HOLLAND'S *Medical Notes and Reflections*, p. 156.

PRACTICAL SUGGESTIONS.

Table I.

Tissues most liable to disease, especially to inflammation, in the order of arrangement:

1. Mucous,
2. Venous, (*venous congestion,*)
3. Cellular,
4. Serous,
5. Ligamentous, and dermoid, (*fibrous,*)
6. Glandular,
7. Lymphatic,
8. Nervous,
9. Synovial,
10. Preriosteum, (*fibrous,*)
11. Osseous,
12. Tendons, cartilage, dura mater, and pericardium, (*fibrous,*)
13. Muscular,
14. Arterial.

Table II.

Parts of tissues most liable to disease, especially to inflammation, in the order of arrangement:

1. Mucous texture, ...	of the nose,	
	" lungs, fauces,	
	" eyes,	
	" small intestine,	Ilium, Jejunum, Duodenum.
	" stomach,	
	" large intestine,	
	" uterus and vagina,	
	" bladder.	

2. Venous texture, (forming, mostly, venous congestion,)
    - of pia mater, in infancy and childhood,
    - " liver,
    - " small intestine,
    - " pia mater of adults,
    - " rectum, (*piles,*)
    - " uterus, (*phlebitis,*)
    - " lungs, (*congestive asthma,*)
    - " lower extremities, (*varix,*)
    - " spermatic cord, (*circocele.*)
3. Cellular texture, ...
    - subcutaneous,
    - of the lungs,
    - " pia mater,
    - " voluntary muscles.
4. Serous texture, ...
    - of the lungs,
    - " parietes of thorax,
    - " parietes of abdomen,
    - " liver,
    - " small intestine,
    - " large intestine,
    - " heart and pericardium,
    - " cerebral ventricles,
    - " kidneys,
    - " stomach.
5. Glandular texture, . . .
    - lymphatic glands,
    - mammæ, (*puerperal,*)
    - salivary glands,
    - liver,
    - testis,
    - lacteal glands,
    - kidney,
    - thyroid gland, (*goitre,*)
    - thymus gland,
    - pancreas.
6. Lymphatic texture, . . .
    - of the lower extremities,
    - " upper extremities,
    - " uterus, (*see Comm. vol.* 2, *p.* 470,)
    - others rarely.
7. Fibrous texture, ...
    - ligaments,
    - dermoid,
    - periosteum,
    - cartilage,
    - tendons,
    - pericardium,
    - dura mater.

8. Nervous texture,	brain, nerves, ganglia of sympathetic, spinal cord. (*See p.* 63.)
9. Synovial texture,	of the knee joints, " ankle, " joints of upper extremities.
10. Osseous texture,	spongy bone, solid bone.
11. Arterial texture,	of the brain, arch of aorta, " extremities, rare in other parts.

TABLE III.

Relative danger of high inflammation affecting the tissues of different organs, according to the order of arrangement:

1. All textures of the brain.
2. All textures of the heart and pericardium.
3. Venous and lymphatic textures of the womb, iliac and other veins.
4. Peritoneum of abdomen, *puerperal women.*
5. Serous membrane of small intestine.
6. Veins of the liver, *venous congestion in congestive fevers.*
7. Parenchyma of lungs.
8. Glandular texture of liver.
9. Mucous texture of small intestines.
10. Mucous texture of stomach.
11. Serous texture of large intestine.
12. Textures of kidney.
13. Mucous texture of large intestine.
14. Serous texture of lungs and thorax.
15. Serous texture of liver.
16. Serous texture of abdominal parietes, (*common inflammation.*)
17. Veins of lungs, (*low, or sub-active, forming congestive asthma. See Comm. vol.* 2. *p.* 494.)
18. Textures of bladder.
19. Mucous texture of uterus.
20. Ligaments.
21. Bone and cartilage.
22. Lymphatics of extremities.

Table IV.

Tissues which require the greatest extent of general bloodletting, when affected with high inflammation, — according to the organs in which they are associated, and in the order of arrangement. The remedy is supposed to be applied early.

1. All textures of the brain.
2. All textures of the heart and pericardium.
3. Serous texture of small intestine.
4. Peritoneum of abdomen, *in puerperal women.*
5. Parenchyma of lungs.
6. Serous texture of stomach.
7. Serous texture of large intestine.
8. Veins and lymphatics of uterus. (*Early.*)
9. Serous and glandular texture of liver.
10. Venous texture of liver. (*Sub-acute, congestion in congestive fever. Often more largely.*)
11. Mucous texture of small intestine.
12. Uterus.
13. Textures of kidney.
14. Mucous texture of stomach.
15. Mucous texture of large intestine.
16. Serous texture of lungs and chest.
17. Serous texture of abdominal parietes. (*Common inflammation.*)
18. Ligaments. (*Often more largely.*)
19. Bladder.
20. Mucous texture of bronchiæ.
21. Mamma, testis, parotid gland.
22. Absorbents of extremities.

POSTSCRIPT.—As a few copies only of the foregoing work have been distributed, I have thought it worth while to take a brief notice of an article with which my "Examination of Reviews" has been honoured in the July number, (1842,) of the *British and Foreign Medical Review.*

Dr. Forbes having hitherto chosen other vehicles than his own Journal for his notice of my "Examination," we have a right to suppose that he would put forth his whole strength whenever he might bring the British and Foreign to bear upon the subject. But the only "point" of defence instituted is the following:

> "He has actually ministered to our gratification on a point generally considered as touching all men most nearly, viz., the improvement of our exchequer. It is a fact which Dr. Paine can any day verify, by a reference to our New-York publishers, that in the quarter immediately succeeding his great advertisement of this Journal, the demand for it in America was increased one twelfth!"—(*Review, p.* 213.)

From what I have hitherto shown of the statements of Dr. Forbes, and from my knowledge of the falling off of his subscription in several instances in this country, I availed myself of the privilege in the foregoing extract, and called upon the publishers to learn how far this attempt to invalidate my "Examination of Reviews" was founded in truth; and since, also, Dr. Forbes assumes that he is a "man of honour" to my disadvantage, I am reluctantly compelled to submit the following facts to the public.

When I sent forth my "Examination of Reviews," I obtained from the publishers of the British and Foreign Medical Review in this country the names of near subscribers for the purpose of forwarding to them copies of the Examination. The list was then very respectable in this city and neighbourhood. But it now becomes my unwelcome duty to state that there are only *one hundred and twenty-one copies* on the Publishers' list, for all North America—the United States alone having more than 20,000 physicians. These copies are distributed in the following manner: State of *New-York* 31 copies, (one being taken by myself;) State of *Pennsylvania* 7; State of *Georgia* 1; State of *Maryland* 22; State of *Rhode Island* 2; State of *Massachusetts* 41; *District of Columbia* 1; State of *South Carolina* 12; State of *Connecticut* 2; CANADA 2 = 121.

It should be also understood that *just prior* to the publication of my "Examination," an effort was made to improve the "Exchequer" by reducing the subscription in this country from *six* to *five* dollars. *Had there been*, therefore, an increase of subscribers, "a man of honour" would have imputed it to THE CAUSE which was *intended* "to touch all men most nearly."

Dr. Forbes is pleased to complain that, I have "carefully transmitted my publications *by post* to all the most eminent men in the profession in Great Britain, and even on the Continent." *Hinc illæ lachrymæ.*

This is in part true, so far as respects my pamphlets; but I have, in nearly all the instances, paid for the freight and duties, not doubting that it would be an indelicacy to pay the small European postage. Until, therefore, I learn from some other source that the postage is onerous, I shall continue the same practice with the present and any future publications; though I flatter myself that no European gentleman will think it amiss should convenience sometimes prompt me to send him a pamphlet through the post-office in this city. I may also add, that my work on the Cholera Asphyxia, of which there were 2,000 copies, and six pamphlets, (besides the present of which there are 2,500 copies,) have been distributed gratuitously. Of my "Examination" I published 2,000. Upon the copies of this pamphlet which I sent to Europe I paid a freight and duties of about fifty dollars, though I apprehend the *nature of the work* was such as to have justified its transmission "by post" from this country. My work on the Cholera was bound, and a more bulky volume, and I therefore, at no little trouble, paid the entire expenses of its transmission to its various places of destination in Europe. I lament the necessity of these details; but it will be seen that they have been rendered unavoidable.

Dr. Forbes is also pleased to refer to my Materia Medica, which, as I state in the "Preface," "was not undertaken till within the last two months, and has been carried on in the midst of many pressing engagements." Indeed, the plan of the work was not conceived till within that brief period, and amongst the many engagements were seven or eight lectures a week, at the opening of the new medical school.

The only objection, however, alleged against the work, is a pretended obscurity of the "Instructions" to the reader as to certain figures and symbols relative to the "combination of remedies," which, as I state in the "Instructions," were introduced "for the sake of brevity and to impress the memory." This plan of condensation for a manual of Materia Medica, and relating only to the "combination of remedies," has met the approval of all, aye, even of "students," so far as my information has extended; and I shall therefore retain it in the next edition. The examples given in the Review, as the reader will find on comparison, are excessively mutilated, trickish, and rendered unintelligible.

Though my relations to a foreign Journal are of such a nature that I cannot refer to those who have honoured me with their correspondence since I published my "Examination of Reviews," I may yet say, that I have had many letters from the first medical philosophers in Europe, commendatory of my Materia Medica, and a multitude warmly sustaining my "Examination." I need scarcely add, that whoever will turn to my Materia Medica will readily perceive that Dr. Forbes has concealed from his readers the whole essential plan of the work, and the manner in which it is executed; and this the more so, as the concealment is accompanied by the declaration that the "arrangement adopted by the author is somewhat different from that of preceding authors, but not so much so as to be very noticeable by his readers." It is true, the names of the "Classes," and the "Orders in the first Class," are stated by Dr. Forbes; but this only with a view of misleading the reader as to the most important part of the work, — all the details of arrangement, by which the relative therapeutic value of all the articles of the Materia Medica is estimated according to experience, and set forth in consecutive order under the various classes, being *wholly original* with myself. A greater artifice cannot be detected in modern criticism.

In respect to the statement that my Materia Medica "is dedicated to the forbearing consideration of Dr. John Forbes, and to his justice and honour," it is hardly worth while, perhaps, that I should say that the work is dedicated to Professor Caldwell.

⁂ In speaking of Dr. Hall's views at page 43, as well as in my remarks upon the nervous power in its pathological and therapeutical relations, I should have referred to what I have said in my "Commentaries" on the subject of Sympathy, vol. 1, p. 157-158, and especially on the important influence of the ganglionic system of nerves in organic processes, and how this system is physiologically related to the brain and spinal cord.

July 26, 1842.

A DISCOURSE

INTRODUCTORY TO A COURSE OF LECTURES

ON THE

INSTITUTES OF MEDICINE AND MATERIA MEDICA,

DELIVERED BEFORE THE

Medical Class of the University of New York,

AT THE SESSION OF 1841-42,

BY

MARTYN PAINE, A.M. M.D.
PROFESSOR, &c.

BOSTON:
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This Discourse is respectfully dedicated to the MEDICAL CLASS attending the Session in the New York University, for 1841-42.

DISCOURSE.

Gentlemen,—It being a part of the duties of my chair to teach you the Philosophy of Medicine, I shall endeavor, at all times, to follow the *simple* path of Nature; and in so doing, we shall find nothing but an admirable system of design, intimately associated in all its parts, always consistent, reaching from the *rudimentary* development of the organic being to the ultimate extinction of its laws, whose multifarious phenomena perpetually spring from principles which are established in the *germ*. Whatever, therefore, may be the complexities of disease, you have the consolation of knowing that it is simple in its *essential* elements, that these elements are *forever* present, and that you will be mainly employed in investigating the *modifications* which they undergo. Here, then, you will always stand upon a great and immutable foundation of Nature. This will always be your guide, always your point of departure, and to this you will always return from your excursions into the paths, which radiate from it in conformity with the constitution of the great plan of organic life.

Nature, indeed, is *always* simple in her *fundamental* laws. She abhors a complexity of causes, as much as she is said to do a vacuum. The Institutes of Medicine, therefore, and the Philosophy of the operation of Remedial Agents, being directly the offspring of the properties and laws of organic beings, *they*, also, must, of necessity, be simple in *their* essential attributes. Hence, it is manifest, that my course of instruction will have mutual and intimate dependencies—forming a connected chain which cannot be interrupted without impairing the whole.

But, there are, also, *artificial* institutes of medicine—such as are not recognized by Nature; and which, therefore, consist of a broken series of hypotheses, either having no relation to each other, or standing in direct opposition. From this, it necessarily results that the treatment of disease, which is founded upon these hypotheses, is not less unphilosophical, not less contradictory of itself, and abundantly demonstrative of the primary error. This has been the misfortune of medicine at all ages of the art, but, at none more so than in our own times. With *these* I must also make you acquainted, though I shall limit my exposure of error to the doctrines of the present day. You will thus enjoy the opportunity of exercising your own judgments, and of making your election between good and evil—myself allowing that the evil may be mine.

In adopting this only method of arriving at the truth, my earnest desire to remove some formidable obstacles from your path will necessarily give an *appearance* of asperity where none is intended. It will unavoidably result from the nature of the contrasts which will be presented; and the importance of our pursuits to the interests of mankind, and to the dignity of mind, will permit no complimentary sacrifice to the ingenuity of speculations. Truth must be stern and inexorable, to overcome the obstinacy of error; for error, like the heads of the hydra, can never be propitiated. We may cut off its heads—but still the principle of regeneration may remain. We must then come to the *actual cautery*, as employed by Hercules in the case of the hydra, as our only infallible resource. But, in all that I may say, now, or hereafter, I wish it to be distinctly understood, that my remarks have no intended application to *men*, but are simply designed for the promotion of truth—of that truth which was so beautifully recommended to your admiration, the last evening, by its "Professor in Theory and Practice."

I shall begin my course of instruction by examining the composition of organic beings in both animated kingdoms—though not so much for the purpose of acquainting you with the abstract fact, as of presenting a series of coincidences at the very foundation of organic beings, which irresistibly demonstrate their dependence upon peculiar properties and laws, about which all physiology, all pathology, all therapeutics, are immediately concerned. A knowledge of these vital properties and the laws they obey, and which, as of all other existences, we acquire through their peculiar and endless phenomena, is fundamental in medicine. They preside over all healthy and morbid conditions. The functions, and all the products of the living being, are *primarily* referable to these properties. Unlike all other properties or powers of nature, they are susceptible of influences from external and internal causes, and are liable to great instability. This instability is at the foundation of disease, and of therapeutics—though the latter involves the important constitutional principle, that when these properties are diverted from their natural condition, they have an *inherent tendency* to return to their natural standard.

We have thus before us, in a *few* words, the simple elements of physiology, pathology, and therapeutics. They are all immediately concerned about one elementary principle, which is manifested under different phases. The *physiological* or *natural* condition is constituted when the vital properties possess their *normal* state; the *pathological*, when they are *altered* from their natural condition; whilst *therapeutics modifies* their morbid changes, and places them in a condition to obey their constitutional tendency to return to a state of health. *This* they are often able to accomplish without the intervention of art, which *never* cures, but only places *nature* in the *way* of cure. This is all that can be understood of the celebrated *vis medicatrix naturæ*, a most important law appertaining to the constitution of the vital properties, but about which much illusion, and many extravagancies have prevailed.

Such, then, is fundamental in medicine. But, however elegant this simplicity in the most essential attributes of organic beings, such are the natural modifications of the properties of life in the different organs and tissues, and such their liability to change from a thousand influences, and according to the nature of those influences, and although nothing happen but under the direction of some everlasting law which operates according to the existing combination of causes, medicine is, nevertheless, the most profound, the most complex, the most difficult of all human pursuits. It was once thought to have been only worthy of the gods—and temples were consecrated to their disciples.

But, the vital properties must have their *instruments* of *action*, through which the *functions* are performed, the being obtains his growth and nutrition, his secretions elaborated from the universal alimentary fluid, and the morbid phenomena carried on. These instruments of action consist of the visible organization; and in every part of this are the vital properties implanted.

It is obvious, therefore, that we can have no adequate knowledge of the functions of life, whether healthy or morbid, without a critical familiarity with the organization through which they are conducted; and this will become more and more apparent as we advance in our inquiries. Nor is it alone a *general* outline of the human mechanism which will enable us to comprehend the arcana of disease, or to apply its appropriate remedies. The vital properties are *differently modified* in every organ, and in the different tissues of each organ, and often in the same parts of a continuous tissue, and they are not only constantly liable to different influences from remote causes, but according, also, to their natural modifications in different parts. They must, therefore, be liable to great differences in their *results;* whether physiologically, pathologically, or therapeutically considered. We must also be familiar with the sources from which the various organic viscera derive their nerves and bloodvessels, and how communications are established amongst the organs by these and other anatomical connections.

The most important of these media of communication is the cerebro-spinal and ganglionic system, especially the sympathetic branch of it. The distribution of the sympathetic nerve should be well comprehended, as should also those cerebral nerves which are contributed to important vital organs, or others which associate themselves with the sympathetic, and thus establish intimate relations between the organs of animal and organic life. By these various nervous connections, the harmony of the entire machine is maintained, or the equilibrium of the whole disturbed when morbific causes may derange the action of one part or another. It is also through these connections, particularly, that remedial agents, whether applied to the stomach or to the skin, exert their salutary impressions upon diseased organs which may be remotely situated. The communicating nerves, however, are often complex, and their analysis is not always of easy attainment. But, a knowledge of the nerves, with which the natural and morbid functions are so intimately associated, is less important than that of the general tissues which compose each vital organ. The peculiar properties of the nervous system, which are known as sensibility and sympathy, have no other participation in organic processes than as they *influence* those processes. The nervous power is to be regarded in the light of a vital stimulus, and regulator of the organic functions—sensitive to an inconceivable degree, and whilst itself a vital stimulus, is capable of *being acted upon and modified in its nature*; but operating in health without any remarkable demonstrations. But, its *constitutional* nature is such for the maintenance of uniform and harmonious movements throughout the animal fabric, that when *influenced* by *unusual* causes, it may undergo a violent development, become *modified* in its nature, and may determine *healthy* or *morbid* changes in the *organic* properties, or *extinguish* them in the *twinkling* of an eye.

I was not a little gratified at the manner in which our Professor of Surgery adverted to the prodigious results which are destined to flow from the discoveries of Sir Charles Bell; and I will go perhaps even farther than he, and anticipate the time when those discoveries will be to medicine what the calculus is to mathematics. Encouraged by the brief, but comprehensive and philosophical reference which was made on Monday evening to the principles which are involved in this great triumph of physiology, I will also venture to predict, that the doctrine which I have just *propounded* for the first time in relation to the modification of the nervous power, will open to us the whole philosophy of remote sympathy in connection with pathology and therapeutics. It opens to us the whole philosophy of the operation of morbific and remedial agents upon organs that are remotely situated from the direct seat of their application. This supposed modification of the nervous power is in perfect harmony with the instability of the organic properties themselves, and is not only sustained by the whole force of this analogy, but by *all* the phenomena that relate to the principle of remote sympathy. That you may, however, the better comprehend what I regard as modifications of the nervous power, according to the nature of the agents or causes by which it is developed, whether natural, morbific, or remedial, you have only to consider, farther, how *electricity* is modified by the *galvanic* apparatus, and how again by the organization of the *torpedo*; how each affects in peculiar modes the properties of life, or how each determines peculiar influences upon inorganic substances; or how, again, perhaps, the magnificent conception may have been demonstrated by our Professor of Chemistry, that the colorific, the calorific, and the deoxydizing rays of light are only modified states of a common substance. Think, also, of the wonderfully different attributes of each of these rays, and you will then have no difficulty, with the aid of the multifarious phenomena of sympathy, and the cerebro-spinal and ganglionic system, in comprehending how this poison or that, or this remedy or another, or joy or grief, shall so *develope* and *modify* the nervous power, that it shall be directed upon, and alter the organic properties of parts that are distant from the direct seat of the morbific or remedial action, in one case in one way, in another case in another way. I know of no exception to the theory, and without it you cannot explain the remote influences of remedial agents; whilst this construction of their *modus operandi* is in perfect harmony with the natural phenomena of sympathy.

But, what may be the *nature* of the nervous power, or of the organic powers, no one can divine, any more than he can imagine the *nature* of the most tangible and ponderable substance. But this does not prevent us from *knowing* the *existence* of the substances, and their *laws*, and that each is *radically different* from the others.

The practitioner, however, may come to acquire, by a diligent observation of nature, an adequate apprehension of the vital influences which one part exerts upon others, without knowing, in detail, the special media of communication. But, his knowledge of these influences is vastly facilitated by anatomical acquirements, pursued with a reference to this fundamental point; and without this anatomical aid, conviction is slow, and success can only be obtained by a long and careful study of the phenomena of life, under their varied aspects of health and disease. This labor, too, with the best anatomical proficiency, must be always great before the laws of sympathy can be justly realized in their variously modified relations, or in their practical bearings.

Rare genius may analyze the phenomena of disease, often reduce them to generalizations, and apply the *appropiate* remedies, without, perhaps, understanding the details of structure. We have examples of this nature among the fathers of medicine, of whom none is so remarkable as he who created the science, and expounded that system of rational philosophy, which, at a distant age, was consecrated by Bacon, and has since given to arts and sciences an impulse which is carrying them fast to the limit of any rapid improvements. But, though it be easy to *admire* the genius which *creates* a science, we may not *imitate* it. Most of us must be contented to study *its elements*, and to advance, step by step, before we can grasp the principles *already* known, and reduce them to practice. Such, at least, has been my own laborious experience; slowly gathered in the field of nature—or by consultation with others—or by unwearied meditation. We must come, therefore, to the work of preparation, not only with resolute purpose, but with a *zeal* that shall maintain that purpose.

Although anatomy is at the foundation of practical medicine, it is only that of the great vital organs with which the *physician* is particularly interested; and yet these are the ones which are most apt to be neglected. Such as relate to that division which is called animal life, the muscles, for instance, is to him comparatively unimportant. It is here that the surgeon holds his empire, and to him this branch of anatomy is not less indispensable than the former is to the physician. The mere operative surgeon may practise his art without knowing whether the stomach be composed of one or more tissues—whether it be seated in the abdomen or in the thorax. And with all this ignorance, he may acquire a far greater temporary renown by a stroke of the knife, than the most consummate philosopher in medicine. But he, who would embrace in the range of his usefulness the more exalted art of *treating* surgical diseases, and who prefers the conservative efforts of nature to the more summary process, must be skilled in the whole department of anatomy, and in the divination and cure of *internal* disease. *So*, will you be told by *one* whom the world *delights* to *honor*.

Considering, therefore, gentlemen, the importance of anatomy to your practical pursuits, I trust you will spare no effort in making an attainment which will exalt you above the ordinary grade of practitioners, and which will bring you a daily recompense in the conviction that your knowledge of disease reposes on one of the great foundations of nature—that your remedies are directed to an intelligible purpose, and, whether the issue be favorable or fatal, that you may equally rejoice in the consciousness that you have been guided by an enlightened understanding of the art, or that your efforts have not been frustrated by the imbecilities or the rashness of ignorance.

We are now conducted to the immediate objects of inquiry in my lectures; the first of which is physiology in its relations to the natural condition of organs. This is the beginning of a *vast* superstructure, which has been the progressive work of many ages, and which, more than any other pursuit, has enjoyed the laborious efforts of genius, and the unwearied toil of a great, though humbler class of mankind.

From physiology, we ascend to its application to pathology, and therapeutics; which, in their connected series, make up the vast fabric that reposes upon the *structure* of organs.

Physiology, according to its Greek derivation and original import, embraces the whole field of nature. The moderns, however, have restricted the word to the science of life. In this acceptation, it comprehends all that relates immediately to the powers and functions of organic beings. It takes in, therefore, the whole vegetable, as well as the animal kingdom. Each of these kingdoms possesses in common the most important conditions of life, though existing in each under specific modifications or varieties; not, however, very dissimilar, and intimately connected by a gradation of analogies, as we descend along the chain of either till we come at their connecting link in the lowest being of one or the other—as the sponge, for instance, which has enjoyed the distinguished honor of having been successively assigned to the three kingdoms of nature, and of being now exalted to the dignity of an animal. Other conditions are superadded to the nobler kingdom, which, with the differences of structure, and the modifications of their common properties of life, and their modes of subsistence, distinguish the two living kingdoms from each other. I shall enter largely into the consideration of these topics, as all rational medicine is intimately concerned with their knowledge. The importance which I have attributed to anatomy is predicated of these ulterior objects. It would be in vain that you comprehend the *structure* of organs, without knowing the nature of the powers and functions which they subserve, and the laws which they obey; since all diseases consist essentially in certain alterations of the properties of life, which lead to all the modifications of the laws and functions, and to every sensible result of a morbid nature. A knowledge of the whole is obtained through the phenomena as they are presented to our senses, and by nothing else. From these, we reason to the invisible existence, its changes, &c., with as much certainty as to those which possess the most tangible characteristics. It is just so in respect to the mind, and the brain with which it is associated, whether in their natural or morbid aspects.

Since, therefore, every disease consists fundamentally in some alteration of the properties of life, you will at once perceive that physiology is the most important element of medical education. Its knowledge necessarily involves that of anatomy, and all practice, which does not constantly refer to the tissues diseased and to the modified conditions of the vital properties, is purely empirical. Empiricism, however, may be of an enlightened nature under the direction of *rare* genius; but, in all other hands, it embarrasses nature, and is a curse to mankind. Of this you will meet with demonstrations in your professional intercourse. You will also occasionally witness the triumph of mind over the most absolute defects of education. In all such instances, however, you will see it glancing at the whole array of symptoms, and forming its conclusions from nicely balanced combinations of the whole with that store-house of experience which is garnered up as a necessary guide to the solution of every new problem. These problems are as various as every case of disease, and as every variation it may undergo during its decline, or in its advances to a fatal termination. Thence is it, that without our fundamental requisites, genius, combined with observation, must be often at fault; and it may be difficult to say whether its success will compensate for its failures and mistakes.

But, qualifications of this order are rare examples, and the most usual consequences of deficiency in anatomical and physiological acquirements, are the most appalling ignorance of disease and a frightful destruction of human life.

Let us call up another large and better class of practitioners—educated, and often erudite men, but who make not a proper application of their knowledge—as was well said of Broussais, on Monday evening. The special evil with this class, if we except the Broussaians, consists in not regarding the properties of life as they are naturally modified in the various textures of the body, and in not considering disease as consisting essentially in morbid alterations of these properties. This class embraces many of the most distinguished men of our age, and their train of followers, especially in Europe, makes up no small part of the profession. They have generally but an imperfect apprehension of the properties of life, and whilst they allow of their existence—nay, more, whilst many of them maintain the extraordinary doctrine of their existence in the *elements* of matter, by the strangest contradiction, and in violation of that fundamental principle in philosophy which prohibits an unnecessary multiplication of causes, they maintain that all the great vital processes, all the secretions, &c., are carried on by the forces which govern dead matter, and of which the *chemical* are supposed to be mainly instrumental.

This doctrine shuts out, *of course*, all true pathology; and yet are these the philosophers who are now most ardently engaged in developing those lesions of organization which result from morbid processes, but in the production of which it would seem to be sufficiently obvious that totally different causes have been concerned, than such as prevail in the inorganic world. Many of them have also gone back to an opinion which prevailed in the dark ages, that all disease consists essentially in a primary lesion of organization, but without revealing to us any more than their benighted ancestors, how those changes of structure come to pass. But, as if for the *purpose* of multiplying causes, and of placing them in the same relative contradiction in which they have arrayed the vital and chemical forces, they have also gone back to the humoral pathology, and whilst they tell us that the essential cause of disease consists in a lesion of organization, which is independent of all agency of the vital properties and of the forces of chemistry, they also affirm that its essential cause consists in a vitiated state of the blood. I will not now tell you of the practical conclusions which have been founded upon this utter confusion of causes.

The *physical* doctrines of life have had their sway at various eras, though especially characteristic of our own age; and the humoral pathology had been an appendage of almost every system till the beginning of the last century. It was then that *solidism* began to rise with the radiant beams of *that vitalism* which gave animation to medicine from Hippocrates to Celsus, and before which the humoral pathology was but as a *withered weed,* until finally, after many centuries of stinted growth, it was plucked up and forgotten. The appearance of Hunter and Bichat swept away every vestige of that philosophy which had so often disfigured the science of medicine. Tiedemann, and other illustrious vital physiologists, followed in the wake. You may know their philosophy by a single passage from the great German physiologist. "Already," he says, "it has been more than once attempted to deduce life from the laws of mechanics, physics, and chemistry. This error has been committed," he goes on, "by physiologists and physicians of the iatro-mathematic and iatro-chemical schools. But, in every age, distinguished naturalists discovered this error and opposed it."

"Among *physical* people," says Hunter, "we find such expressions in *common* use, as, the humors are affected in the blood; sharp humors in the blood; the whole blood being in a bad state; the whole blood must be altered, or corrected; and a variety of such expressions, *without meaning*. They even go so far as to have hereditary humors, as *gout, scrofula*, &c., and make us the *parents* of our own humors, saying that we *breed bad* humors. In short, the *whole theory* of disease has been built upon the supposition of humors in the blood, or of the *blood itself being changed.* I cannot conceive," he adds, "what *is* meant by it." But, what Hunter avows he could not comprehend, is now consecrated as the *science* of medicine.

But perhaps Hunter was dull of apprehension, though he studied organic nature more extensively, and more laboriously, than any other man, before or since. If we consult the opinion of Bichat upon the same physical doctrines of life and disease, we shall still find that their successors are apt to consider dulness of apprehension to exist in the ratio of genius and observation. "To *what errors* have not mankind been led," says Bichat, "in the employment and denomination of medicines? They created *deobstruents*, when the theory of *obstruction* was in fashion—and *incisives* when that of the *thickening* of the humors prevailed. The expressions of *diluents* and *attenuants* were common *before* this period. When it was necessary to *blunt* the *acrid* particles, they created *inviscants, incrassants*, &c. Those who saw in diseases only a *relaxation* or *tension* of the fibres, the *laxum* and *strictum* as they called it, employed *astringents* and *relaxants. Refrigerants* and *heating* remedies were brought into use by those who had a special regard in diseases to an excess or deficiency of caloric. The same *identical* remedies have been employed under *different names*, according to the *manner* in which they were supposed to act. *Deobstruent* in one case, *relaxant* in another, *refrigerant* in another, the *same* medicine has been employed with all these opposite views; so true is it that the mind of man gropes in the dark, when it is guided only by the wildness of opinion."

But, what Bichat thus describes as having only *successively* prevailed at different eras of medicine, is now bodily incorporated into the science, and constitutes, in Europe, especially, its whole essential feature. Were this kind of medicine truly founded in nature, you readily perceive that it would be useless for me to do more than simply to state the facts, and that my course of instruction might properly terminate with this introductory lecture. There would be *no principles*, *no institutes*, *no laws*, *no variety* to expound; and we might lie down at once with Brandreth and Morison.

Bichat, having drawn the portrait of his predecessors and of many cotemporaries, which I have just exhibited to your observation, is then led to apostrophize:—"Hence," he says, "the *vagueness* and *uncertainty* our science presents at this day. An *incoherent* assemblage of *incoherent* opinions, it is, perhaps, of all the physiological sciences, that which best shows the caprice of the human mind. What do I say? It is *not* a science for a *methodical* mind. It is a shapeless assemblage of inaccurate ideas, of observations often puerile, of deceptive remedies, and of formulæ as *fantastically* conceived, as they are *tediously* arranged."

Such, then, was also Bichat's obtuseness of apprehension. But, gentlemen, neither Hunter, nor Bichat, nor Tiedemann, nor any of their great compeers in the investigation of nature, were the *dunces* which the hypotheses of our own day would declare them. An *Augean* work was then accomplished by a *stream* which may be now and then obstructed, but which will forever break up the barriers, and sweep away the offals that may accumulate in the dry channel below.

Hunter expounded, more amply than his predecessors, the doctrines of life, and founded upon them the only true systems of pathology. His masterly analysis of inflammation exemplifies the whole range of disease, and its philosophy lies at the foundation of the whole; though I by no means intend to imply that all diseases are inflammatory. But, if it be true that inflammation is essentially constituted by morbid changes of the vital properties and functions, then may the same affirmation be made of every other deviation from a state of health. This will be rendered apparent hereafter, when I come to speak at large of the laws and the analogies of nature. It was Hunter, also, who first disclosed the modifications or peculiar conditions of the properties of life in their relation to different organs, and the different tissues of the same organ.

Scarcely had this extraordinary man disappeared, when Bichat took up the great subjects, and carried the whole world before him. His doctrine of life, and the pathology which is founded upon it, recognizes no physical agencies beyond those foreign causes which maintain the vital powers in operation, or which convert them from their natural to morbid conditions. He analyzed the vital principle more extensively than had been done by Hunter, and though deeply indebted to this philosopher, he pays no tribute to his unexampled labors or his exalted services. But nothing can impair the claim which mind establishes to its own property. It is as immortal as the spirit which gives it birth; and though it be for ages entwined in the laurels of others, it will ultimately light on the memory of him who enriched mankind in enriching himself.

Bichat, however, makes the capital and contradictory mistake, like most other vitalists, of regarding life as an *effect*, or as consisting of the assemblage of those phenomena which result from the operation of the vital properties, in their connection with the instruments of action. This construction, as we shall see hereafter, is not only philosophically wrong, but practically bad. If, for instance, life be made up of the *functions* of organization, we should regard disease with a simple reference to the functions, and these are so clearly *effects*, there would be no tangible cause through which our remedies might operate, whilst no office appears to be assigned to the admitted vital properties. But, as there must be clearly *something* altered from its natural state anterior to functional derangement, we must allow that the primary cause consists in a change of the properties which preside over the functions.

Bichat, however, was sometimes inconsistent, and perhaps more so upon the great subject before us than upon any other; for, although he endeavors to show that life is constituted by the functions, he argues that disease (which is only an unnatural state of life), is constituted by a morbid change of the vital properties. Nay, in the following extract he makes life itself to consist in the vital properties, and regards the functions merely as *effects*, of which the vital properties are the *cause*. Thus:—

"Examine," he says, "*all* the physiological and *all* the pathological phenomena, and you will see that there is *no one* which cannot be ultimately referred to some one of the vital properties of which I have just spoken. The *undeniable truth* of this assertion," he goes on, "brings us to a conclusion *not less certain* in the treatment of diseases—namely, that every curative method should have for its object the restoration of the *altered vital properties* to their natural type. Every remedy, which, in local inflammation, does not diminish the augmented irritability; and which does not diminish animal contractility in convulsions, and elevate it in paralysis, fails in its object, and is contraindicated."

Here, then, Bichat teaches the philosophy which will be fundamental in *my* lectures. It was essentially at the foundation of all *his* pathological writings; and it is therefore the more remarkable that he should have been so speculative and contradictory when treating specifically of life.

Bichat's career was brilliant, and though dead at 31, he lived, like Hunter, to enjoy the ripest fame. He was, however, but a meteor-light; dazzling for a moment, and *then* extinct. He was one of the last of a galaxy, who had so illuminated the field of medical philosophy, as left but little else for the aspirations of ambition—following the beaten path of nature—than to accumulate facts and to arrange them under established principles. This occupation is too humble for the restive ardor of genius, and too servile for the purposes of renown; and facts had already amounted to an encumbrance. It is not, therefore, remarkable, that when the great work had been brought near the verge of completion, giants should spring up to overthrow the fabric, and erect a new edifice upon its ruins. The revolution began simultaneously in different parts of Europe, and under different aspects. But, so many powerful and ambitious minds had been in operation for ages, ingenuity had not only exhausted fundamental principles, but every imaginable hypothesis. The former being the last in the series, it only remained to reproduce exploded and forgotten doctrines. The most important of these were the physical and chemical doctrines of life, and the humoral pathology. Chemistry, too, was now in the ascendancy amongst sciences; and the brilliant discoveries which it was pushing in the inorganic world promised a harvest of *fiction*, if not of *fact*, in behalf of the crude hypotheses of darker ages. Physiologists, therefore, became practical chemists, and chemists became speculative physiologists; and for more than twenty years past, the study of organic life, and the philosophy of disease, have been mainly carried on in the test-glass and crucible. The blood, the secretions, and every part of the animal fabric, have again and again passed the ordeal of the laboratory, in the vain expectation of discovering the springs of life, or the essence of disease. The laboratory now copes with nature in its artificial compounds for the digestion of food, and the very furnace is brought into operation to manufacture a fluid which, it is pretended, is not inferior to that product of the stomach which results from an organization as various as the species of animals, and according to their habitudes in respect to food, and whose contrivance for this specific, variously modified, *vital* fluid, required the Mind of an Almighty Being. And this is but a *fair* example of the modest ambition of chemistry.

But, *who* are the philosophers that thus invade the sanctuary of life? Learned, laborious, and useful; but are they familiar with organic beings? Do they study their phenomena? Can they tell you a *stomach* from the bladder, when both are before them? Can one in a thousand distinguish pneumonia from enteritis? The replies are too obvious to be stated. They *live in the laboratory*, which, in their estimation, monopolizes all the vitality that is worth a philosopher's attention. They will solve you any of the most hidden secrets of organic beings. Are you curious to know how the various unique constituents of the bile are elaborated out of blood? Nothing, say they, is easier. Here are they all—picromel, cholesterine, asparagin, ozmazome, resin, bovin, oleate, acetate, margarate, cholate, bicarbonate, phosphate, sulphate and hydrosulphate of soda, potash, &c.—all here in the blood—when, in simple truth, not one of them have an existence in that fluid. *So* is it admitted by some of the chemists, and so is it proclaimed by the laws of organic beings. They are no more in the blood than is the poison of the viper or the ink of the cuttle fish.

Then the *admirable* simplicity of the manner in which we are told these exact constituents are separated from the blood to make up the bile —whose final causes illustrate so strikingly the evidences of design, is worthy our special notice; though it may be rationally supposed, that since the constituents are assumed to exist in the blood, it is also assumed that they are merely mechanically strained off by the liver; whose organization is as various and as specific as the hundreds of thousands of animals whose species are distinct. It was undoubtedly owing to prevailing doctrines of this nature, that many distinguished chemists, whom I have quoted on another occasion,* have let slip the severest censure of the chemical and physical doctrines of life—even such as practise organic chemistry in defiance of their acknowledged and direct opinions to the contrary. They universally allow, indeed, that "the laws of inorganic chemistry are *utterly inapplicable* to the phenomena of life;" and that, though "there is a chemistry of life, of that chemistry we know *nothing*." It is therefore all assumption; and this reputed "chemistry of life," of which it is admitted the chemist "knows NOTHING," is exactly the thing of which the physiologist professes to know SOMETHING.

Confining ourselves to philosophers who are entitled to our profound respect, you will readily concede that Bichat comes far within the limit which is here prescribed, and that his opinion should *also* weigh in proportion to the decision with which it is given. Let us, then, hear the great French philosopher.

"The organic chemistry of the *laboratory*," he says, "is the *dead* anatomy of the fluids, not a *physiological* chemistry. The physiology of the fluids should be composed of the *innumerable variations* which they experience according to the different (vital) states of their respective organs." "The instability of the vital powers is the *quicksand* on which have sunk the calculations of all the physicians of the last hundred years. The habitual variations of the *living* fluids," he adds, "*dependent* on this *instability* of the powers of life, one would think, should be no less an obstacle to the *chemical* physicians of the present age."

"Again, had *physiology* been cultivated by men *before physics*, I am persuaded that many applications of the former would have been made to the latter. Rivers would have been seen to flow from the tonic action of their banks, crystals to unite from the excitement which they exercise upon their reciprocal sensibilities, and planets to move because they mutually irritate each other at vast distances. All this would appear unreasonable to us, who think of gravitation only in consideration of these phenomena. And why should we not in fact be as ridiculous when we come with this same gravitation, with our chemical affinities and chemical compositions, and with a language established upon their fundamental data, to treat of a science with which they have *nothing whatever to do.* Physiology would have made a much greater progress, if all those who studied it had set aside the notions which are borrowed from the accessory sciences as they are termed. But these sciences are *not* accessory; they are wholly strangers to physiology, and should be banished from it wholly." "To say that physiology is made up of the *physics* of animals, is to give a very absurd idea of it. As well might we say that *astronomy* is the *physiology* of the stars."

It has ever happened, as an inevitable consequence of the fundamental alliance between healthy and morbid processes, that, whenever chemistry, or physics in a more general sense, has invaded organic nature, the philosophy of disease and of therapeutics has followed in abject submission.

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* See Medical and Physiological Commentaries, Vol. 1st, pp. 36—40, 75, etc. Vol. 2, p. 114—122, etc.

I cannot now conduct you through the astonishing details, which, taken one by one, would show you that organization—aye, that intellectual being whose divine portrait was so vividly drawn on Tuesday evening—is *practically* regarded, as it is *theoretically* pronounced, a mere chemical laboratory,* composed of tubes of various calibre, amenable to the laws of chemistry, which, it is imagined, may yet be imitated by the hand of man, and that with the aid of the vital properties in the elements of matter (and of course, by analogy, the soul also), whose primeval existence in this relation is maintained for contingent purposes, the confident hope is promulgated that the laboratory will yet be able to exert the highest Prerogative of Creative Power. Already, indeed, it has been given out, *ex cathedra*, that nothing is easier than the accomplishment of digestion by artificial mixtures, in the preparation of which, as I have said, the *furnace* is sometimes instrumental—that nothing is easier than the artificial fabrication of those organic compounds which are elaborated by an *unfathomable* organization that was designed for this specific purpose by the Divine Mind—but, *not* till the alimentary material has been vitally decompounded and its elements recombined in a mysterious manner by the gastric juice—then subjected to the farther organizing effects of the bile and other organic products—passed through the wonderfully vivifying lacteals—carried forward and subjected to the whole animating influence of the pulmonary system—perfected in its exalted endowments by the whole labyrinth of the circulatory organs—and lastly, though not least, determined from the blood in one everlastingly exact manner by other complex living systems—but *how*, no imagination can form the most remote conception, but through the instrumentality of those specific properties of life, which were the only power concerned from the beginning to the ending of the astonishing series of unvarying changes;—and, as might be inferred from the utterly groundless pretensions, aspirants have lately appeared with *animals*, composed of nerves, stomachs, respiratory organs, eyes, ears, *instinct*, &c.—both male and female, as on a former occasion, and *now*, as *then*, made out of the elements of matter—though in the modern case, *galvanism* is the *creative spirit.* These offsprings of the ingenuity of man, it is true, after having been duly added to the trophies of chemical science, were ultimately suspected of having been the work of another Power, and even by those philosophers who emblazoned their scientific journals, in Europe and in America, with pictured illustrations of these supposed creations of the laboratory, and who once "*saw that it was good.*" But, I will not neglect saying, that the pretended creation, independently of the living organization, of any one real organic compound, however *simple*, is not less fallacious, and arrogant, and irreverent, than was the pretended creation of insects out of silex by the spirit of galvanism.

I shall not farther speak of the moral and religious tendencies of these speculative invasions upon nature. They must be sufficiently obvious to every reflecting mind; however they may be offensive to one, or admired by another. My present interest lies in their *philosophical* merit, and in their practical bearing upon health and disease. They are uprooting every intelligible principle which relates to the former, and are converting the latter into the confusion of chaos. The humoral pathology, especially, has ranged itself under the auspices of the all-potent science; and, although as wide spread as Christendom (if we except the common empiric who has never relinquished his hold upon the same doctrines as taught during the long decline of medicine in the dark ages), its modern ascendancy has been, as it were, the work of a day. Davy, having accomplished his wonderful discoveries in the mineral kingdom, the credulous, the zealous, or the ambitious, built upon these achievements a hope that something would turn up for the benefit of a favorite hypothesis in medicine, or something for the advancement of reputation. The world was crowded, beyond example, with *learned* men, and with an *interminable* audience—constituted morally and intellectually just as they were when Pope wrote his "Essay on Man" and the "Temple of Fame," and that other delineation of men and manners, which is called the "Dunciad."

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* See my Medical and Physiological Commentaries, Vol. I., p. 36.

After a long and laborious investigation, however, of the imputed connections of chemistry with organic philosophy, I have seen no reason to qualify the declaration which I have more publicly made, that I know of *no solitary fact* contributed by chemistry, which has thrown a ray of light upon the philosophy of life or the arcana of disease; and, as concurring in this sentiment I have been most happy to refer to the opinion of my learned associate who presides in the inorganic kingdom, and who so justly surrenders to the physiologist an interpretation of the living department of nature. We may therefore hope for an enlightened co-operation from this philosopher, in my efforts to place you on the right road of inquiry, should he think it sometimes expedient to travel out of his inorganic dominions to regale himself at the *fountain of life*. I shall therefore have less hesitation in speaking of the illusory and *seductive* nature of the *experiments* in organic chemistry, and more especially of their application to the *laws* of organic beings—so seductive, indeed, that they beguile our senses, and cheat us of our understandings. You must look upon all these doings as upon the arts of legerdemain—apparently real, but most grossly deficient in their pretended elements. Notwithstanding the recent triumphs of chemisty in the inorganic kingdom, and the vast multiplication of its powerful resources, it has left the whole science of life and disease just where it abandoned them when it was in a comparative infancy—or, with only the difference in the relative amount of error, and the exertions which are now necessary to its exposure and defeat.

But, of this subject I shall speak more fully hereafter, and shall only now add for the purpose of securing your attention to the important questions before us, that the farther chemistry pushes its investigations, the more it multiplies proofs that the whole subject of life and disease belongs to another department of philosophy. We find, indeed, that chemistry is everywhere against the hypotheses which have been founded upon its own principles; and, in this negative sense, it will have *greatly* contributed to the science of life. All that is now necessary to obtain the full benefit of the light which has been thus reflected, is the permission of chemists that it shall take its natural direction. But, it has been said with a justice applicable to all ages, that "every new system of philosophy, true or false, must be embraced and introduced into medical science." Asclepiades explained all by the Epicurean or corpuscularean philosophy; Galen and his disciples by the philosophy of Aristotle; another eminent sect by the mechanical philosophy of Newton, who, says Bryon Robinson, "discovered the cause of muscular motion and secretion, and furnished materials for explaining digestion, nutrition, and respiration"—whilst Sir Humphrey Davy and numerous followers also think it "possible that one law alone (of a physical nature) may govern and act upon all matter; a law," continues the great chemist, "which might be called the law of animation." Hence, it was well said by Bichat, that "chemists and mechanical philosophers, accustomed to study the phenomena over which the physical forces preside, have carried their spirit of calculation into the theories of the vital laws." The principle is well exemplified by the metaphysician, Dr. Reid, who says that "Mr. Locke mentions an eminent musician, who believed that God created the world in six days, and rested on the seventh, because there are but seven notes in music. I myself," the Doctor continues, "knew one of that profession who thought that there could be only three parts in harmony, because there are but three Persons in the Trinity. A chemist imagined that he had the felicity of having discovered a principle (not that of Newton's or Davy's), which would expound all the phenomena of organic beings. The physiologist, after listening to his philosophy, told him that there was but one circumstance adverse to his discovery, which was, that the physiological facts were all exactly the opposite of what he had supposed. The chemist then begged the physiologist to state what the facts were, that he might *explain* them by *his* system. And, to the same effect we have the opinion of Lord Bacon, who says of Cicero, that "he went about to prove the sect of *Academicks* to be the *best*; for, saith he, ask a *Stoic* which philosophy is true, he will prefer his own. Then ask him, which approacheth *next* the truth, he will *confess* the *Academicks*. So deal with the *Epicure*, that will scarce endure the Stoic to be in sight of him; so soon as he hath placed himself, he will place the Academicks next him." The reasoning of Cicero is as good for the physiologist as for the Academicks—for so soon as the iatro-chemical or the iatro-mechanical philosophers have placed themselves, each, and all other sects who build up a spurious philosophy of life, will place the physiologist next.

The metaphysician, Brown, admonishes us emphatically against the propensity of carrying the theories relating to favorite pursuits into other sciences. And thus, Lord Bolingbroke, as if in rebuke of Reid, and Locke, and Brown:—

"Metaphysical writers," he says, "counsel us sometimes very gravely to silence imagination, that we may attend to experience, and hearken to the voice of reason. The advice is good, and they would neither puzzle themselves, nor perplex knowledge, if they took it as they give it."

This is the evil; and as well said by Mr. Lawrence, "what we are to guard against in our professional researches and studies, is the influence of partial and confined views, and those favorite notions and speculations which, like colored glass, distort all things seen through their medium." We must build upon *facts*, and facts *alone*. Nor is this *all* that is necessary. We must have the *last* as well as the first in the series; for the *last* fact may be necessary to determine the proper application of the whole, and establish a sound generalization, or *theory* as it is called. *Hypothesis*, on the contrary, rests upon a *partial* array of facts; and this is the reason that, whatever is hypothetical grasps at a thousand shadows, and perverts a thousand realities. But, in no inquiries is the mind so apt to go astray, and to carry its hypothetical conclusions into other departments of nature, as in the science of chemistry. Here, everything seems demonstrative, and yet everything may be essentially deceptive. The enlightened chemist will confess you this; and whilst he fears that the fabric of inorganic chemistry may be overthrown, he hopes to be more permanently associated with organic nature. That he is right in his fears, every day is supplying proof upon proof. Sometimes the proof is positive, sometimes negative; and of the latter we have just had a remarkable exemplification in the proclamation by the celebrated Professor Christison, that he had converted the compound substance known as cyanogen, and renowned for the mischief it has done in organic philosophy, into the simple element called silicium, and which is not only a simple substance, but utterly different from either of the elements of cyanogen. This is only an exemplification of the bold positions which are now rapidly taken by chemical philosophers; and, had it turned out as represented by the professor, it would have struck a fatal blow at every principle in chemical science. Indeed, upon the strength of this supposed metamorphosis of nature, a learned friend told me that it was not improbable that the halcyon days of alchemy were about to be realized in a substantial manner, and that we should soon have our furnaces for the transmutation of iron into gold, and the famous "tincture of all-flowers" into the never-failing "elixir of life."

It therefore ceases to be remarkable, that chemistry should have pushed for laurels far into the labyrinth of organic life. The perpetual blast of the furnace, however, the frequent jeopardy of life and limb from explosive mixtures, and the pursuit of other devices *by day* and *by night*, to turn the whole organic kingdom into the laboratory, can leave no doubt that what may have been originally the prompting of ambition grows into enthusiastic delusion. We see, therefore, the puzzle of the philosopher who "observed to Crito how unaccountable it was, that men, so easy to confute, should yet be so difficult to convince. Make a point never so clear, it is great odds that a man, whose habits and the bent of whose mind lie in a contrary way, shall be unable to comprehend it." Nevertheless, "we have among us moles that dig deep under ground, and eagles that soar out of sight. We can act all parts, and become all opinions; putting them on or off with great freedom of wit and humor."

But, is there no *fundamental* guide which may enable the inquirer after truth to perceive, at the glance of an eye, the wide gulf which separates chemistry from physiology? A gulf so vast should be everywhere studded with insignia in all its surrounding outskirts. I have already told you of many; but I will now show you the *gulf itself.*

Inorganic nature is *at rest.* Its great characteristic is *vis inertiæ.* Here, then, are no phenomena to denote the forces and laws by which its internal constitution is governed. But, it so happens that chemistry may set its forces in motion, overthrow its composition, examine its elements, and elicit a train of phenomena which declare its fundamental laws and forces. These, therefore, are proper and necessary experiments, since they are concerned with the forces of nature, and are the only mode by which we can reach their phenomena.

Let us now turn to the other side, and see how it is with *organic* nature. In all things *exactly* the reverse. Here, everything is in *motion*—in *creative* motion. Its powers and laws are open to the observation of all, through their perpetual and endless phenomena—and to which there is nothing remotely analogous in those results which are obtained by the chemist when he sets in motion the forces of the inorganic world.

In the laboratory, then, we have experiments upon nature in her state of *torpidity.* In the organic body we have the experiments of *nature herself.* Consider, too, that in the former case, they are meagre, uncertain, and at the mercy of every breeze. In the latter—in the individuals of every species, they are inexhaustible in variety, and in all the hundreds of thousands of species varied according to the varieties of organization—but all concurring to demonstrate a near identity of forces and laws; and coming *directly from* nature, they *cannot deceive.* Will you, therefore, prefer the experiments of man upon organic nature when deprived of its peculiar properties and laws, and subjected to forces unknown to the organic being—and worse than all, when that being is broken up in its very structure and elements? Do you not see the absurdity of such distortions of nature? Are you not rather contented with *her own* endless experiments—so endless that you may unceasingly study them for the span of your life, and yet you shall have only *entered* upon their variety. What other experiments can we require than such as are thus perpetually presented by the organic being—varied as the species, varied as every moment, varied as disease from health, and the phenomena always true to the fundamental laws? Or, if something may be yet *artificially* elicited, should it not be done through the *living* organization, that its *own* appropriate forces and laws may have *their* share in the extorted results? Do you not instinctively answer, yes? I was *certain* that you would, and have so written it down.

Here I had intended to have made a hiatus in my discourse; but the patience with which you have listened encourages me to persevere to the last. I have been admonished by kind friends, within and without the profession, that a doctor's discourse should never trespass beyond the good old limit of an hour; and this being my first address to a public audience, I had almost determined to surrender my wonted habits of thinking for myself. But it certainly appears to be an established rule, that a professor of medicine can hazard only an hour—whilst the more bountiful allotment of two hours is assigned to the parson (when he chooses to take it), six hours to the lawyer, and from twelve to forty-eight hours to a member of Congress. Whilst each keeps himself within the limits, respectively, the rule is—never to *leave the room*, nor to *snore aloud*. For myself, I shall only ask for "the benefit of clergy."

I will now cursorily glance at some other mischievous consequences which have resulted from the restoration of the physical doctrines of life. One of the most important, and most productive of evil, is the prevailing hypothesis which assigns, as the cause of inflammation, a stagnation or interruption of the circulation of blood in the small vessels, which carry on the processes of disease; and this doctrine is now extended by distinguished writers even to idiopathic fever. It takes away all agency from the vital properties, all function from the instruments of disease, and resolves all the remarkable, unique, and diversified phenomena of those two great classes of disease, which swallow up all the important human maladies, upon purely physical principles; as physical and as lifeless as if the being were positively dead. The hypothesis, therefore, offers no light to the practitioner, nothing to guide his hand, no cheering consciousness that he strives with a disease which the *mechanic* could not as well control. But, we shall find, gentlemen, that it is all exactly otherwise, and that these diseases which make up the great amount of human suffering, and form the principal outlet of life, are under the same great laws which determine all *healthy* processes—only, however, partially modified by certain primary alterations of the properties of life. Were the mechanical doctrine true, of what use to us would be our knowledge of physiology? Where would be its application to disease? It would have no remote bearing upon the subject, and the whole scheme of pathology could be written out upon a quarto page. But, the vital properties, in inflammation and fever, so far from being paralyzed, as it is called, are exalted in power, altered from their natural state, and are the fundamental cause of all the phenomena that are seen or felt. The blood is neither stagnant nor coagulate; but moves in the instruments of disease with increased velocity, and in an augmented quantity. With *these* facts before us, there is something for *philosophy* to contemplate, something consonant with the laws of life, and something to encourage the practitioner in a rational treatment and with the hope of success.

Are you anxious to know the origin of a doctrine so derogatory to philosophy, so contradictory of fact, so subversive of all rational principles in medicine? I will tell you, then. Like all our other prevailing physical hypotheses, the mechanical doctrine of inflammation is only the ghost of darker ages—shorn, however, of what was originally considered its animating and indispensable attribute. It was the conception of one Vacca, an Italian physician of vivid imagination, who never *pretended* that it rested on a *solitary* fact. It was considered, indeed, so utterly baseless, that Hunter does not refer to its existence. But, what was thus *originally* the project of imagination, *now* professes to rest upon *experiment*. It is also a curious coincidence, that all the exploded doctrines of antiquity which have been recently brought forward to decorate an age which *boasts* of its *originality*, never were advanced under even the *pretended* auspices of fact. But, as I have already said, the most remarkable appendage to Vacca's hypothesis, and which the inventor considered indispensable, is studiously kept out of sight.

Vacca maintained a debility of the bloodvessels, in consequence of which they were said to lose their power of propelling the blood, and, as another consequence, the blood is supposed to stagnate and coagulate within them. So far his followers. But here *their* pathology stops; and as to their principles of cure, they are of course as mechanical as the pathology. But, a great majority do not even allow of independent action to the bloodvessels, in their *natural* state, but refer the whole movement of the blood to the propelling power of the heart, and perhaps, also, to hydraulic pressure. They only recognize, therefore, in inflammation, a mere physical relaxation of the coats of the vessels—just as leather is relaxed by soaking in water, and probably much in the same way. Their diameters being thus enlarged, the current of blood is said to stagnate like water in the wide channels of muddy and shallow rivers—this being, *verbatim*, *their* philosophical comparison.

Vacca, however, had the sagacity to perceive that mere passive relaxation of the vessels, and stagnation of blood, would never explain the exalted temperature of the part inflamed, and its various other morbid phenomena. He therefore boldly assumed that a real combustion, an absolute fire takes place in the blood as a consequence of its stagnation in the vessels; nor have we any other ground for this opinion, than that *inflammation* signifies *a fire*. There are, he says, four principal fluids in the body; namely, the blood, the serum, the fat, and the nervous fluid. The serum, he says, is too watery to burn, but the blood burns tolerably well, and the fat burns after its well-known manner. This, you will also probably surmise, is the origin of our doctrine of spontaneous human combustion—which is one of the present embellishments of physiology. The nervous fluid is said by Vacca to be so volatile that it escapes the conflagration; and it is left *undecided* whether it be combustible or not. It therefore remains a *fair* subject for experimental inquiry; and it is difficult to divine why it has been so utterly neglected by the chemist. It is also a fundamental principle with our projector of the now prevailing doctrine, that no inflammation can take place without the *presence* of atmospheric air to *ignite* the contents of the bloodvessels. The antecedent stagnation, he maintains, lets in the atmosphere, which draws the inflammable parts into the vessels, and there ignites them. The tumefaction of the part is said to be considerably owing to an evolution of gas generated by the process of combustion, and this swelling gives room to a farther ingress of combustible matter.

Vacca affirms that these are *essential requisites*, and that without them there can be no inflammation. He published this nonsense in 1765, in a work entitled "De Inflammationis morbosæ Natura, Causis," etc., and its mechanical part is the now prevailing doctrine of inflammation; whilst one of its *vital* consequences, pus, is considered, as it anciently was, a mere chemical decomposition of the tissues inflamed.

Why is this doctrine so extensively embraced? Because it is captivating, like Brown's and Broussais' theories of disease, by a simplicity which exempts the mind from all laborious reflection, either as to the remote causes, the pathology, its contingent influences, or the mode of treatment. But, with Vacca's embellishment, there was a factitious analogy with the immense latitude over which the science of disease naturally stretches. There was, at least, abundant room for the riot of imagination, and something to give a show of plausibility to the stinted *mechanical* part of the hypothesis. Living Nature, gentlemen, is full of poetry, and man gets *all* his best poetry from her—just as the physiologist obtains from her all his doctrines of life and disease. But, as there is a poetry of the imagination as well as of nature, so, also, are there *imaginary* as well as *real* physiological doctrines. Those which are *real* are the natural *poetry*, as well as the *basis* of medicine—and they shrink, *instinctively* as it were, from all physical and mathematical calculations.

There is *another* wide spread and fatal disease, which I regard as inflammatory, and upon the philosophy of which I shall have something to say hereafter. It has attracted but little attention either in respect to its pathology, or treatment, but which, perhaps more than any acknowledged inflammatory affection, is supposed to be under the dominion of physical laws. This disease is *Venous Congestion*; appearing under simple forms, or complicated with idiopathic fever. In the former case, it exists as an independent affection of the veins, but constantly liable to involve other tissues, or the whole system, in sympathetic influences. When connected with *idiopathic fever*, it still maintains the character of a local and distinct disease. The two, co-existing, mutually influence and exasperate each other, just as do local inflammations of other tissues and idiopathic fever, when they co-exist.

In respect to Venous Congestion, it is remarkable that even during the ascendancy of *vitalism*, or when pathology was generally considered in its true relations to nature—it is remarkable, I say, that even then, venous congestion was regarded in a mechanical sense. It was then, as now, supposed to depend upon some obstruction to the venous current, and a consequent stagnation of blood in the congested veins. Since the general revival of the physical doctrines of life, this disease has attracted more attention, and has been more extensively expounded upon mechanical principles. The vital properties and vital actions have been universally excluded as elements in its pathology; and it has served as a *recruiting force* to the analogous pathology of inflammation. Remedial agents have been therefore applied upon physical principles, and their effects, if salutary at all, are construed in conformity with the same philosophy.

Considering, then, that inflammation, fever, and venous congestion, comprise most of the maladies we are required to treat, it may be safely said of medical science, that "there is nothing stirring but stagnation."

Nevertheless, I shall ultimately show that congestion of the veins, like all other diseases, falls under the common law of dependence upon an altered state of the vital properties of the venous parietes—that there is no obstruction, no stagnation of blood in the case, but that it flows in the congested veins as freely as in health. The philosophy of this disease is of vast magnitude, since it is scarcely less prevalent than the common forms of inflammation, whilst it is more complex in its influences upon the system at large, of far more difficult treatment, and much more fatal. It forms the predominant feature in the yellow fever, and in the congestive fevers of this climate, and throughout the southern and western States. It is the great source of their obstinacy, and the main cause of their fatality.

You hear much, gentlemen, of the great advances of medicine in recent times. And so it has advanced; but only so in the accumulation of facts. There is scarcely one physiological, or pathological, or therapeutical doctrine now advocated by the "*reformers*," as they call themselves, which was not more or less in vogue at degenerate ages of our science. Whether they be anatomical, chemical, or mechanical, they have all had their day, and have all been exploded as utterly contradicted by the phenomena of life and disease, and by all that is known of organic philosophy. And this I say, as due to the great cause of which I am an humble advocate.

It is not here, however, on American soil, that those seeds of darkness have taken root. With a few rare exceptions, our own medical philosophers have gone on cultivating philosophy. You will not soon forget that spirit-stirring reference, which was made by our professor of surgery, to the revolution of empires—and upon which, as I imagine, as well as upon the facts which I have now announced, he founded his conclusion, that the city of New York may yet be destined to supply Europe with her medical philosophers; and that, too, not unlikely, within a century hence.

But, there is one physiological heresy of which I have not spoken, and with which we have been also favored by the physical speculators, which surpasses all others in its degrading tendencies—for it overthrows the science of physiology and medicine at its very foundation. Like all the rest, however, it was a doctrine of the dark ages. It appears to have had its revival in the *laboratory*, though not *exactly* within the prerogatives of that modest handmaid of Nature. It has, however, won its way extensively into medical favor, and chemistry is, as usual, thanked for the blessing.

This doctrine supposes that the fluids circulate in the small vessels by *capillary* attraction—just as oil ascends in a lamp-wick, or water is imbibed by a sponge. So we are told by Liebig, for instance, the great organic chemist, and by many others. The doctrine, I say, is necessarily subversive of all physiological, pathological, and therapeutical principles—since it is one of *mere mechanics*. All the important vital processes being carried on by the small vessels, it must be perfectly apparent, upon the doctrine of capillary attraction, that nothing of a vital nature can be performed by these vessels. In short, I know of no doctrine so derogatory to medical philosophy as this one of capillary attraction.

The ignorant *pretender* will tell us that all this is unimportant; though no one is so much directed by hypothesis, or theory, as this very pretender himself. Does not every empiric in the land prescribe his drastic cathartics for the purpose of cleansing the blood of its supposed impurities? Are they not exactly on a par, in their doctrines, and in their practice, with the most speculative of our enlightened humoralists? Nay, have the ignorant portion of that sect, our Brandreths, our Morisons, *et id omne genus*, any reference whatever to facts or experience? Is it not all hypothesis, and, therefore, all a reckless waste of human life? How is it with the homœopath? Certainly all hypothesis, and never a fact but such as demonstrate his errors—if nothing worse. Mount up the scale, and you shall find at every step of your ascent, from him who *prowls* about the outskirts of the profession, to him who directs the all-potent drug with the most consummate skill, that each and all mainly rely upon their conceptions of the *philosophy* of disease. But you shall also find, that in proportion as Nature has been taken for their guide, and as medical principles are founded upon the absolute phenomena of life, in their healthy and morbid aspects, *there* will always be the greatest reference to facts and experience. How momentous, then, that we should follow Nature, and that our theories should be derived from her observation alone.

The human mind *will have* its theories upon all subjects; and the whole history of medicine is a perpetual exemplification, that in no inquiries do theory and hypothesis abound so universally as in the healing art. This arises, in part, from the intricacies of the subject, but mostly so from the constitution of the mind itself. The Almighty *designed* it for theoretical conclusions, and set us the *example* in those stupendous Theories upon which the Universe, and all it contains, are founded. And what else are, or should *be*, *our* theories, than finding out and adopting those of which He is the Author? What other theory in the *natural* world can there be, than *such* as are instituted by the Almighty Being? And shall we hesitate to embrace, and to act upon *such* theories? And yet it is one of the improvements of our day, to insist upon nothing but facts, and to denounce all principles in medicine; as if the Almighty had not ordained principles and laws as well as facts—which are mere emanations from the former.

But, who are they that would thus convert our noble and stupendous science into its barbarian infirmities? They are the greatest theorists of the age—promulgating their speculations under cover of this pretension. This propensity of the mind to theorize is strikingly illustrated, for example, in the writings of Louis (a distinguished Parisian physician), who, although condemning theory and generalizations in medicine, is the greatest speculatist of any era; nay more, he has embodied in a work which purports to be a simple record of facts, a greater number of hypotheses than can be gathered from the whole field of medical literature.

We *must*, therefore, have theories in medicine; and, therefore, let us have the *right* ones. Right or wrong, they grow irresistibly out of the constitution of the mind and the fundamental laws of nature. Let not the mind indulge its great natural propensity without a constant reference to those laws, through the medium of their phenomena. The elements of the former are simple, immutable, and easily known by their manifestations. These manifestations are the facts, and form the substantial ground of all intellectual acquirements. As they relate to organic beings, to their laws, their properties, their functions, whether *morbid* or *healthy*, they are to be found in the organic being *himself*—not in the workshops of the chemist or of the mechanical philosopher. But, even where the mind admits this proposition, if prone to speculation, it too often regards each fact by itself, and rears up hypotheses wrong in themselves, and in conflict with each other. Facts should therefore be compared before they are reduced to theory; or, where they may conflict with acknowledged principles, they should remain in an isolated state till their true nature may be better understood, or till the principles which they appear to contradict may be shown to be erroneous. Had this consideration been duly regarded, had the Attributes of the Almighty been properly respected, or the thousand facts in physiology, our age had not been stained with animal magnetism.

Should you meet with some fact which appears to indicate the dependence of life upon chemical or any other physical forces, the evidence to the contrary is so various and conclusive, that *that* fact must be considered as deficient in some of its elements, which, if known, would readily bring it under a well-established principle in physiology. These absent elements are some other facts which escape our observation, perhaps through necromancy or imposture; and thus what is truly fact, in an abstract sense, is made the groundwork of important error. And did those of you, who venerate the Mosaic Record of Creation as the Word of God Himself, never entertain a *hope* that Geology may yet discover *other* facts which shall bring *such* as are known into better harmony with the Word of God? May we not believe, as we shall soon see has been often the case with hypotheses founded upon partial facts, that a *solitary* discovery may yet show us that our geological premises have been deficient in a most fundamental element? Should we not *tremble* over the ruins of about one hundred theories of Creation, which, by a recent decision of theoretical geology, even in the metropolis of France, are pronounced "unscriptural and unworthy of record"? Would it not be safer to *rest* upon our facts, and be contented for the present to *know*, that "In the beginning God created the heaven and the earth"; and, in believing this, to think it also *possible* that the subsequent annunciations are equally true? It strikes me, at least, that this is not only the safer, but the *philosophical* course.

But leaving sacred, for our more appropriate subjects, there are principles which are not as clearly confirmed by an observation of nature as the laws of life; and, in such instances, it may be that the supposed principle and the conflicting fact should mutually stand the ordeal of inquiry. This will be accomplished by a full revision of the facts of which the principle had been predicated, and by the multiplication of other facts. It may be found that they do not all harmonize with each other, or it may happen, as with organic beings, that there is a perfect coincidence. In the former case the principle is *prima facie* false; in the latter, it is *prima facie* true; but neither induction will be certain till the newly discovered fact is reconciled to those upon which the principle had been founded, or is shown to be in absolute opposition. In the former case, the principle stands, and derives farther confirmation; in the latter, it is more or less shaken, or may be overthrown and the facts become assembled under a new doctrine.

It sometimes happens that the discovery of a new fact will overthrow the most brilliant theory. Had Christison succeeded in that higher pretension than was ever made by the alchemists—that of converting cyanogen into silicium, he would have upset the whole science of chemistry—and in *this* respect he would have rendered a service to physiology. In the instances, however, to which I am now referring, the theory is generally of a compound nature, and some of its elements rest upon facts which nothing can invalidate. In such cases, also, the facts are of a demonstrable nature, and that which invades the theory is clear, specific, and liable to no uncertainty. Lavoisier, for instance, laid down the doctrine that oxygen gas is a *supporter* and the *only supporter* of combustion. The former part of this doctrine must remain forever true; the latter was only good till some other substance should be discovered, which, like oxygen, would maintain combustion. It was so far a hazardous principle, as it was concerned about abstract facts, and might or might not, therefore, be a fundamental law of Nature. The very next revelation of the laboratory might show that this part of the theory was a mere assumption—as it certainly was. A *single* fact was only necessary to the purpose; and already not less than three other agents are known to be supporters of combustion. Some have even supposed that all cases of intense chemical action, where heat and light are developed, are instances of combustion; and then we have spontaneous human combustion, for which no theory has been assigned. But, the universal doctrine, which respects heat and light abstractedly, rests principally upon the two facts just stated, and is otherwise deficient in the analogies which relate to true combustion. It is, therefore, like Vacca's doctrine of inflammation, and that of spontaneous human combustion, probably nothing but an assumption.

Again, it was supposed to be a law that oxygen was essential to *acidity;* and although it be generally true that this substance is the acidifying principle, others are now known to exist. Here, this great agent placed the same theorist in another predicament corresponding exactly with the calamity which befel the doctrine of combustion. The theory was partly true, and partly false; whilst its universality was overthrown by a single fact. In all such instances, where the laws have no great range of phenomena, it is unphilosophical to theorize beyond the absolute facts in possession. But, here also, other theories, of the same latitude and uncertainty as that which supposes combustion in all cases of intense chemical action, when light and heat are developed, have sprung up—some chemists supposing that acidity often arises from the associated effect of several elements.

In the examples before us, therefore, we not only see how readily certain doctrines, which rest upon abstract facts, may be overthrown by a single discovery, but with what readiness the mind starts off upon hypotheses when opportunity arises for the exercise of ingenuity. It is the peculiar misfortune of science to generalize too hastily; and it often happens that the explosion, or the introduction, of one error, is the parent of many others. It is also astonishingly true, as we have especially seen of the doctrines of life and disease, that a few phenomena are abstracted from the whole, of which they may be only sequences of the others, and are made the ground of doctrines which are in perfect conflict with other and better theories that are instituted upon the *more fundamental* facts;—and thus a blind disregard of consistency is permitted to prevail, till a most incongruous series of assumptions is presented to us as the science which Nature teaches.

Although facts are the only foundation of theory, it is not unfrequently the case that certain existences, and the laws by which they are governed, may be fully demonstrated without any knowledge of the *nature* of the fundamental subject to which they refer. This, for instance, is true of light; for, although we know not the condition in which it exists, or whether it produce its impressions by impulses and oscillations, or by projections, &c. (from near or remote objects), the laws of reflection and refraction are permanently fixed. The same affirmation may be made of electricity, and the laws which this remarkable agent obeys. And so, also, of heat. These laws, and those in relation to light, are founded upon such facts as cannot be shaken; and when, therefore, apparently conflicting phenomena may arise, we may be certain that they will be ultimately reconciled to the established principles. Least of all can any theory of the *nature* of light, heat, or electricity, or of the modes in which they are developed, affect the laws which have been founded upon their phenomena. And though it be possible that light, electricity, and heat, are modified states of a common substance, their phenomena, and the laws which are predicated of those phenomena, declare that some peculiar, but unknown imponderable substance exists, upon which those phenomena depend. They declare it to be *sui generis*, differing as much from all things else in Nature, as was its distinct and specific Act of Creation, when the Almighty said—"Let there *be* light, and there was light." We *know* it to be different from every other existence, because it is distinguished from all others by its phenomena and laws.

Just so is it, gentlemen, in respect to the powers and the laws of organized beings—the whole animal and vegetable kingdoms. It matters not whether the principle of life, whose elements we denominate the vital properties or vital powers, be ponderable or imponderable, tangible or intangible, or, like the soul, immaterial; for, like the soul, and light, it has its infinitely diversified and peculiar phenomena, and its peculiar laws. Like the soul, and the principle of light, therefore, it must have a real existence—as real as was that other specific Act of the Almighty Being by which He superadded the vital principle to man, when He breathed into his inanimate structure the breath of life;—and therefore, by analogy, by Unity of Design, and by some analogous process (of which the foregoing annunciation is probably metaphorical for its greater intelligibility), into all other organic beings. How stupendous the conception—how corroborated by all the phenomena and laws of life—*how atheistical the doctrine which engrafts those vital properties upon the elements of matter, that they may rob the Almighty of His highest of all Prerogatives—the creation of living, intelligent beings!* And may it not be that the announcement of the creation of "the breath of life," *subsequently* to the institution of the organic structure, was especially intended to prohibit this very doctrine which ascribes to the elements of matter the essential requisite for organizing themselves?

We may be ignorant of the *principle* of life, yet understand its whole government; and the objection is perfectly futile, that we cannot reason about that principle because we cannot demonstrate its nature. Will you deny the existence of the soul because you cannot see it? Will you deny the Almighty because the eye cannot see Him that made it? What else do we know of the most tangible substances, than that they exhibit certain phenomena? Did not Berkeley reject the testimony of his *senses*, because he could not comprehend the *nature* of matter? But, did not *consciousness* compel him to recognize the immaterial soul, when he denied the existence of the *body* which it inhabits? Do you go to Revelation for your proof of an Almighty Being? Then, by the same rule your faith must repose upon the declaration, that man was first created an inanimate structure, and that animation was superadded as a distinct Act of Creation. Take either ground, Revelation, or the phenomena of Nature, and you must be consistent. Here, as in most things, Revelation and Nature mutually illustrate and sustain each other. Their annunciations are equally direct upon the subject before us, and open to the understanding of all. Our conclusions, therefore, flow irresistibly from *whichever* premises you may select.

Although it be rather premature, I will carry on my illustration in respect to life, by supposing the existence of some principle analogous in its material nature to that of electricity, or light, though essentially different in its constitution. Grant this fact, and skepticism is at once dissipated. You *see* and *feel* the thing, and yield to your sight and touch where you would not to thousands of demonstrations which are less likely to deceive. You grant the principle of light as an imponderable substance, because it impresses the sight, and this is your only natural proof of its existence. But, when *this* solitary proof is withdrawn by the interposition of the moon between us and the sun, your belief in the existence of an universal elastic medium, capable of being again rendered luminous by solar impulse, is in no degree affected. You go on to believe, though you do not even *see*, and have nothing but a dead analogy to impress the conviction.

Supposing, then, that organized beings possessed a principle of life that could, like light, be *seen*—they would then be allowed to be governed by this agent, and we should be relieved of the encumbrance of the physical and chemical hypotheses. But, though no such principle address itself to the sight like electricity or light, its existence is far more variously and conclusively attested by other phenomena. These phenomena, results, or facts, determine also the nature of the laws which prevail throughout the animated kingdoms: and, being wholly different from such as rule in the inorganic world, it is *prima facie* evident, that powers or properties of which they are predicated, carry on the processes of health and disease. But, it is not *analogy* alone which forces this conclusion. The facts of which it is affirmed are incomparably more numerous and specific than those which appertain to all other powers of Nature; whilst the scrutiny of ages has never produced a fact in opposition.

Indeed, with so much light upon our subject, so much of fact to substantiate our conclusions, it would seem highly probable that all facts which may be raised in opposition have no relative bearing, and that they are brought forward in the spirit of hypothesis.

The more comprehensive a law may be, the more readily is it known and determined, and the less likely is it that apparently conflicting facts will arise. Whenever such are produced, it is owing to a want of proper investigation. The facts are examined superficially; and the speculative or the credulous mind seizes upon some prominent characteristic, and pushes its opposition to nature under the spur of novelty, or the delight of discovery, or the goad of ambition. This, as we shall ultimately see, is emphatically true of the application of chemical forces to the processes of life, and of the more strictly physical to the interpretation of disease and therapeutics.

Let us now apply these remarks in the way of another brief illustration. When Crawford promulgated his doctrine of animal heat, which was founded upon chemistry, it *should have been* obvious that his *indispensable* facts were only assumptions; since all analogy in relation to organized beings rendered it in the highest degree probable that chemical agencies have no lot in the function of respiration, or in the production of animal or vegetable heat. The properties of life are too universally concerned with the results of organic beings to admit the probability that Nature is so inconsistent with herself—or, rather, the Almighty with Himself, as to have instituted a great system of government for the special economy of the organized kingdoms, and at the same time have admitted the forces of inorganic matter to determine *some* fundamental result; and that result, especially, having intimate alliances, and close affinities with all such as clearly depend upon the vital principle.

Crawford's doctrine, however, prevailed almost universally, till it was finally shown, by the chemist himself, to be defective in the necessary facts. Chemistry then started off in pursuit of other hypotheses of animal heat that should be conformable to its own habits and prejudices. It elaborated a now prevailing doctrine that heat is evolved by the conversion of the fluids into the solids, with some mysterious connection with atmospheric air. But, it overlooks the perfectly subversive fact, that adult warm-blooded animals have an uniform and exalted temperature, and that an exact equilibrium is preserved between the conversion of the fluids into solids and of the solids into fluids, whereby the temperature of all adult animals should be regulated by that of the surrounding atmosphere; whilst in infancy, the temperature is lower than in adults, although nutrition overbalances secretion. These facts are irresistibly conclusive against the hypothesis, and are one of the numerous examples in which chemistry has introduced into organic philosophy doctrines which are in total opposition to its own well-established laws. Other hypotheses, of a similar nature, have sprung up upon the ruins of Crawford's—neglecting all Unity of Design, sifting the facts for such only as are plausible, regardless of all the opposing phenomena of life, and scouting the grand principle in philosophy which forbids an unnecessary multiplication of causes. Before this invasion of chemistry upon the vital doctrine of organic heat, the phenomenon was expounded upon purely mechanical principles, as digestion had been; it being supposed to arise from the friction of blood upon the circulatory vessels. Here, however, was something which was merely contingent, and in no respect involving a violation of principle; and I would far sooner take this palpable error, than the absurdities of the laboratory.

It will be a part of my *agreeable* task to exhibit the fallacies of the physical hypotheses of life and disease, as well as to inculcate principles which exalt our science above the mere world of matter, render it consistent in all its details, and present it to your attention as a department of knowledge fundamentally distinct from all other pursuits. Then shall you feel the quickening influence of a philosophical knowledge which distinguishes you from the rest of your race—of a knowledge which led the great father of our art to affirm that "a philosophical physician is like a god"—when you shall have some ennobling glimpses at a system of principles and actions of which the profound in other sciences have no just conception, and which you *alone* are qualified to direct to a great and specific result.

And this carries me again back to the essential philosophy of disease. Assuming that morbid actions are carried on by the forces which govern the natural functions, we may rationally conclude that every pathological change consists in some new mode of action which has been induced in the vital powers by morbific causes, and that the object of therapeutics is to restore the natural condition of those powers. When, therefore, we hear that inflammation, fever, or venous congestion, are constituted by stagnation of blood, and that all their results are interpreted by physical agencies, we may be certain that such hypotheses have no foundation. But, allowing these remarkable exceptions to the ordinary course of nature, what would science be worth, what its advantages to mankind, when thus surrounded by exceptions which cover the whole fabric with doubt, and which divest the most important diseases of all ground for any intelligible treatment?

There is no practical pursuit, in which consistent and philosophical theory is so important as in medicine. Every practitioner, as I have said, is irresistibly influenced by theoretical views of disease, and none more so than they who are most ignorant of its merits. How important, therefore, that our first theoretical conceptions should be *right*—since, being right or wrong, they will be either for good or for evil. Where medical doctrines are not laid upon the broad basis of Nature, or where mechanical or chemical philosophy is allowed to usurp the place of *vitalism*, you will commonly find that theoretical views, and the application of remedies, are at the mercy of every prominent symptom. As new symptoms are constantly rising as the disease acquires exasperation, the hypotheses and the treatment undergo the most contradictory changes—being often within a few hours in absolute opposition.

Thus, gentlemen, you perceive that neither the poorest nor the best of us can move without *theory* as well as experience for our guide; and it behooves us, therefore, to lay well the foundation of medical doctrines. Whether true or false, they will surely operate; and nothing is more difficult than to correct the errors which we imbibe in the course of a medical education. It is with a view to the importance of these objects, that I have addressed you in this general manner in my first lecture, as well, also, to give you some apprehension of the objects of my course, before we embark upon a consideration of the Materia Medica, which I shall teach you in its special relations to medical philosophy. The field over which we shall travel, is of boundless extent, but is everywhere marked by prominent outlines. These outlines I shall be mainly employed in presenting to your attention, under the scrutiny of a vigorous analysis. They have all an intimate association—beginning in simplicity and ending in unfathomable complexity; yet always true to the simple elements, and always determined by immutable laws. Beginning with what is simple, we shall ascend, step by step, to what is complex—till at last, and along a chain of the closest analogies, we attain the most intricate of the whole, and which embraces every part of our plan—the consideration of remedial agents, and their just application to disease. I shall endeavor, therefore—feebly it is true—to teach you the Institutes of Medicine as they are founded in Nature, and with an undeviating view to the Materia Medica. And that this great ultimate object of all medical acquirements should have been taught in our schools apart from the Institutes of Medicine, has always appeared to me an artificial and unnatural separation. I know not, indeed, how the Materia Medica can be intelligibly taught without being associated with extended instruction in the principles of physiology and pathology, to which the investigation of every article should have an unceasing reference. Isolated from these, the Materia Medica can, at best, consist only of a dry detail of facts, without a spark of the animation of which it is susceptible, with no associations to illustrate its vast and endless relations to disease, or to connect them with memory—nothing to govern their therapeutical application, but the monotony of an empiricism as sickening as the drugs themselves.

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A LECTURE

ON

THE IMPROVEMENT

OF

MEDICAL EDUCATION

IN THE UNITED STATES;

INTRODUCTORY TO A COURSE OF LECTURES

IN THE UNIVERSITY OF NEW YORK.

BY MARTYN PAINE, M. D.
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INTRODUCTORY LECTURE.

Art. V.—*A Lecture on the Improvement of Medical Education in the United States; introductory to a course of Lectures in the University of New York for the session of* 1842–3. By Martyn Paine, M. D., M. V. H. P., Professor of the Institutes of Medicine and Materia Medica, in the University.

Gentlemen,

"There is no reputation for genius," says Quintilian, "to be gained by writing on things which, however necessary, have little splendor or show. The height of a building attracts the eye, but the foundations lie without regard. Yet, since there is not any way to the top of science, but from the lowest parts, I shall think nothing unconnected with the art of oratory, which he that wants cannot be an orator."

A disregard of the important principle inculcated by Quintilian in the extract which I have just made, and erecting edifices upon crude and inharmonious materials, or without even the shadow of foundation, have been the prolific causes of the present vacillation in medicine,—of the innovations which are made upon its most solid principles,—of the heresies which come as substitutes from the laboratory of the chemist,—of the "uncertainty" with which it is said to be cursed by nature.

The "imperfection of medicine" has become the watchword of a thousand ambitious minds; and there still lingers on our ear the bold and inconsistent declaration that the Science "is now in its infancy," yet susceptible of "mathematical certainty."* But, worse than all, the infant chemistry, which came into existence ages after medicine had nearly reached the maturity of manhood, having suddenly assumed the proportions and the strength of Hercules amongst his fellow gods, vociferates its pretensions as the legitimate parent of medicine, has driven cultivators from the field of organic nature; and its claim of paternity, which was once regarded as the most farcical stride of ambition, is now conceded to the more gigantic stature and overbearing deportment of this young scion of knowledge.

It has been well said, that "the opinions prevalent in one age, as truths above the reach of controversy, are confuted and rejected in another, and rise again to reception in remoter times. Thus the human mind is kept in motion without progress. Thus, sometimes, truth and error, and sometimes contrarieties of error, take each other's place by reciprocal invasion."* But, most of all, let me admonish you against the illusion which professes to have attained by a few simple observations, or through the chimeras of hypothesis, what it avows to have been the fruitless work of the most profound philosophers, and the closest observers of nature, for nearly three thousand years.

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* See my Medical and Physiological Commentaries, Vol. ii., p. 679—815.

Let me not, however, be misunderstood. It is in no respect my design to undervalue the science of chemistry. The position which I have hitherto taken is defensive only. It is against chemistry when hunting for laurels in the field of physiology, of pathology, and of therapeutics. Thus far chemistry is a selfish aggressor, and may not complain that its encroachments are repelled without compromise, and without sympathy. In its appropriate walks none have greater delight than myself. They are decorated with gems of rich magnificence, of the rarest value, fascinating to the eye, luxurious to the imagination, fertilizing to the mind. Chemistry abounds, also, with sublimities, and overflows with useful inventions and discoveries. The more profoundly, also, we become imbued with a knowledge of the institutions of chemistry, the more shall we revolt from those schemes which engraft upon organic life the laws of a department of nature that are forever in conflict with it, and which ultimately lay waste the whole fabric of organization, and extinguish its ruling and creative principle,—till, at last, they shall reign triumphant over an universal ruin, and extend more and more widely the gulf which now separates organic life from the inorganic world. But, whilst there yet lingers on this or another globe a solitary being endowed with the principle of life, that being may hurl defiance at all the ordinary agents that may have contributed to the waste by which it is surrounded. The globe we inhabit, the whole heavenly system, are but designed for the abode and the uses of life. Those are but gross and subsidiary ; this, like its author, creative of organization, and associated with an immortal spirit whose capacities alone elevate the temple of its birth to proportions corresponding to its own divine nature.

I would, therefore, urge you to the diligent culture of chemical science, as a boundless source of intellectual improvement, for indispensable habits of critical inquiry and exact analysis, to enlarge the high prerogatives of our art, and to enable you to discern, as with the light of the sun, the utter estrangement of the laws which preside over the two great kingdoms of nature. And, in the language of Burke, let us never forget that "WHEN EDUCATION TAKES IN ERROR AS A PART OF ITS SYSTEM, THERE IS NO DOUBT BUT THAT IT WILL OPERATE WITH ABUNDANT ENERGY, AND TO AN EXTENT INDEFINITE."

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* Johnson's Preface to Shakspeare.

It is only, therefore, to the application of chemistry to the lofty problems of organic life in its healthy and morbid aspects, and to the imbecility which would receive from the chemist—from him who lives in ignorance of organic nature, and is only concerned about the elements of matter—a system of philosophy applicable to living beings, and as that philosophy may be modified by disease, that I would offer an objection. " Let a man," says Locke, in his work on the Human Understanding, " be given up to the contemplation of one sort of knowledge, and that will become every thing. The mind will take such a tincture from a familiarity with that object, that every thing else, how remote soever, will be brought under the same view. A metaphysician will bring ploughing and gardening immediately to abstract notions;—the history of nature will signify nothing to him. A chemist, on the contrary, shall reduce divinity to the maxims of his laboratory, explain morality by *sal*, *sulphur*, and *mercury*, and allegorize the Scripture itself, and the sacred mysteries thereof, into the philosopher's stone."

That this is no hyperbole, even at our day, take a passage in proof from Liebig's Organic Chemistry applied to Physiology and Pathology —a work just from the press.

" Physiology," he says, "has sufficiently decisive grounds for the opinion, that every motion, every manifestation of force, is the result of a transformation of the structure, or of its substance; that every conception, every mental affection, is followed by changes in the chemical nature of the secreted fluids; that every thought, every sensation, is accompanied by a change in the composition of the substance of the brain." (P. 8.)

" Chemistry applied to Physiology and Pathology," as it is absurdly called, is the infatuating genius that lays wait your path, and may beguile you into the worst and inextricable errors. It presents you a system of insinuating simplicity—promises you an exemption from intellectual toil—divests pathology and therapeutics, as well as physiology, of every intelligible principle—substitutes itself for all medical knowledge—overthrows every other department in the science—corrupting even its own principles—and renders the practice of medicine "as mechanical as the art of the shoemaker." It is this interference of the laboratory which has shaken so extensively the foundations of medicine, and which so cruelly debases the science. It is said that " there is a sort of gloss upon ingenious falsehoods, that dazzles the imagination, but which neither belongs to, nor becomes, the sober aspect of truth."

The various results of this unnatural enterprise have been intensely interesting to myself, and have engaged no little of my time, as well as my pen. The late publications of Liebig, the acknowledged chief in organic chemistry, will do much to awaken physiologists from their lethargy, or dispel the illusion which has seized upon them. From this time, organic chemistry will be on the wane. It has received a most fatal blow from its greatest champion. I may also say that his "Chemistry applied to Physiology and Pathology," has called up an efficient advocate of truth in the last number of the North American Review. The reviewer and myself are side by side in almost every opinion upon the important questions before us. The difference between us is, he states conclusions only, whilst I had argued the whole, and corroborated each one by a multitude of facts. This I had done even in anticipation of the present crisis. I commend this reviewer to your attention. His article should be read by every physician in the land, especially for the sake of the summary extracts which are presented. The citations alone from Liebig's work will turn every mind from the phantom which has hitherto beguiled the unreflecting part of our profession. You may learn their universal import from a single remark of the reviewer, as well as from my preceding quotation from the author.

"We have purposely given," says the reviewer, "our author's theory of disease and remedies almost exclusively in the author's words, lest any analysis of it should have seemed to our medical readers distorted or exaggerated. We do not think so unfavorably of the understanding of any reader, medical or non-medical, as to believe that there is one among them all who will require a word of comment to convince him how utterly unfounded and extravagant it is. If there be one such, he is past our remedy." "Such," the reviewer goes on, "is the splendid production upon which the British Association felicitates itself, which a Professor of King's College counts it such a glory to have introduced into the British world, and in regard to which there is so violent a contest for the honor of assisting at its birth on this side of the water." (October, 1842, p. 498.)

I say, therefore, that DRY LAND APPEARS, though it be yet far in the distance. It will give animation or courage to others who have hitherto looked on with *indifference*, or in *dismay*, or in *despair;* and we may even expect to be "ENCUMBERED WITH HELP WHEN THE SHORE SHALL HAVE BEEN REACHED IN SAFETY."

But it is not my present purpose to dwell on this extraordinary convulsion, or on its disastrous consequences to the welfare of man. They exist,—are sustained by a powerful array of numbers, and will be pushed till *fruitless ambition* shall take its natural course through other avenues to fame.

May it not be possible, however, to paralyze the operation of error, and bring into general diffusion the light of truth, ere the former shall have exhausted itself, or may scent a new game, and start on another chase? You say, perhaps, that the remedy for error is its exposure through the press; and in part, you say truly. But this is only a small part of the requirements of truth when besieged by its unrelenting adversary. What, indeed, would avail the clearest demonstrations of the most profound philosopher, did he not have willing and enlightened readers, or an attentive and cultivated audience? And medicine, as you will ultimately know, is the most difficult of all the sciences.

Here, then, is the great foundation upon which the superstructure can be alone erected. Knowledge must be carried through the wide domain of our profession. Its *elements*, however, can never be sufficient for the exigencies of the healing art,—can never resist that plausible simplicity which throws its fascinations around the chemical and other physical doctrines of life and disease, and will forever shrink from the stupendous and intricate philosophy which constitutes the true science of medicine. You must, therefore, aspire at the goal of medical philosophers, before you may comprehend or enjoy the philosophy of medicine. You must have willing minds, and steady purpose, before you will study the elaborate writings of its sages, and therefore, before those efforts of genius can bear with any practical effect upon the dogmas of ambition, or the errors of ignorance and superstition. "Books," says Bacon, "can never teach the use of books;" and I may add that in proportion as they are concerned about difficult principles, so must the practical habits of life, and experimental observation through the wide domain of Nature, be brought to their interpretation, and an exposition of their uses. The mind must gather instruction through the same channels from which books of instruction have been derived, before we may comprehend the nature of the latter. Then, shall you go on, with books and experimental observation hand in hand, to provide yourselves with a work of your own creation that shall surpass the most elaborate writings of any sage of our profession. For though, as said by Fenelon, "we admire with reason the invention of books, in which are presented the histories of so many facts, and which are the depositories of so many thoughts, what comparison is there between the most delightful book, and the brain of a learned man? There can be no doubt that this brain is a far more precious collection, and a much more beautiful invention than any book. In this little reservoir you can find, at any moment, every image that you desire. You call them, they come. You send them away, they hide themselves, we know not where, and others appear in their place. We open and shut our imaginations as we open and shut a book; as one may say, we turn over its leaves, we pass suddenly from one end of it to the other. We have even tablets in the memory, to indicate the places where certain images may be found. But, these innumerable characters, which the mind can read so rapidly, leave no traces on the brain. If you examine it, you see only a soft substance, a sort of cluster of fine and tender threads, and mostly water." If you analyze it with fire and acids, you get nothing but the mere elements of matter. The PRESIDING GENIUS evades the grossness of sense, alike in the hands of the physiologist, the anatomist, the chemist, and the medical philosopher.

Such is the book of books; and would you make the highest attainments in medicine, it should be a compound of many written books, and replete with inscriptions by the hand of Nature. It may not, however, be easy to indicate the amount of knowledge or of intellectual labor which is necessary to form a medical philosopher. This will depend much upon natural endowments, upon opportunities of observing nature, and the mode in which those opportunities are employed.

You are inducted over the threshold of medicine by the standard elementary authors; and you are farther initiated by lectures at our public institutions. But, at the very outset, you meet with error as well as with truth; and it is only by systematic perseverance, by rising from elementary authors to the lecture-rooms of medical teachers, where principles are variously examined, and ascending to the elaborate writings of the great masters of our art, and then by applying to the whole the test of your own observations of nature, that you can separate what is true from the false, and acquire that proficiency in medicine which ensures the greatest practical success.

These propositions are too plain to be controverted; and it now only remains to ascertain whether there be any conditions of society which modify the requirements that are enjoined upon physicians by the highest and holiest dictates of the healing art?

Wherever man exists in a state of society, *there* will always be the professional ministers of relief to his suffering and disease. This is even true of the wandering Arab, and of the most abject and superstitious of our race. To them, nothing accrues but the havoc of ill-directed means, or, at best, the hope that is inspired by incantations and charms. These are the lobelia empirics of our own land; and whilst we regard with sorrow the unhappy destinies of our benighted brethren of the woods, or of the hovel, the victims of an obstinate delusion, where knowledge and christianity prevail, scarcely deserve a passing sympathy.

Since, therefore, practitioners of the healing art, whether for good or for evil, must abound in all assemblages of men, it is obvious that provision should be made for the diffusion of medical knowledge as far commensurate with the vastness and the difficulties of the science, and with its relations to the human welfare, as the circumstances of each society will admit. There should be no limit to those requirements which humanity enjoins, and which education supplies, unless imposed by the common exigencies of society; and these are alone of a pecuniary nature. Where wealth abounds, as in the various states of Europe, medical education should be carried to the very stretch of the principles which I have suggested; and it is a scandal to their society that imperfections are tolerated in any shape. The diffusion of wealth should insure to every country in Europe an overflowing profession, conversant with every useful department of science, and accomplished in the philosophy of medicine.

This, I believe, is, to a large extent, an undoubted proposition—so undoubted that there are not wanting many who would make this triumph of wealth a standard of requirements in our young and comparatively necessitous republic. Here, too, our institutions go for the equality of man, and legislation proceeds upon this conciliatory, however unfounded, principle. Whilst, therefore, medical education may be encouraged by provisions that do not conflict with the fundamental declaration of equality of rights and intellect, there will ever remain, whilst the doctrine remains, a passive, but licentious indulgence of the lowest quackery. Such will not only be the inevitable result of a principle adopted from policy, and which did better cause than the artillery of war, but the whole people being the arbiters of principle as well as of law, the majority will be slow to relinquish a concession so flattering to pride and so inflammatory to ambition—and, as you know, there will still be multitudes who will ride into power upon these infirmities of our nature, and who, when once in power, must sustain the fiction which gave them elevation. However, therefore, what is good in our republican institutions may surpass all other systems of social compact, we may make up our minds that medical quackery will receive, at least, the indulgence of our legislators.

This fundamental evil separates us still more widely from European states than the wealth and the leisure which give them an ascendency in literature and arts; and whilst, therefore, the latter may hold replenished the profession of medicine in its highest advances, their legislators, without fear and with a good conscience, may extinguish a pestilence which is more desolating to the human race than that "which walketh in darkness," or all the ravages of war.

Thus, then, we destroy the parallel which has been attempted between our own and European states, and show it the merest fiction of the visionary mind. Exact from our physicians the intellectual culture, and rear in this land the high standard of medical acquirements, which are so noble and fascinating in some of the schools of Europe, and quackery will reign almost universal from one end of the continent to the other. Is not the whole multitude, whether rich or poor, pressing forward either for greater wealth, or for the pittance of their daily bread? Nay, more, do not all our medical colleges hold out the temptation of moderate fees, and give, in their annual announcements, a conspicuous place *to the humble charges for the necessaries of life?** But what are these, compared with the expenses attendant on the prolonged and higher grades of academic and medical learning in some of the European states? And who does not see the inconsistency that would hold in one hand professions of cheapness to allure the student through our present system of medical discipline, and threaten with the other augmented fees and an impossible exaction upon time? The same principle, too, runs through all our primary schools, our academic, collegiate, legal, clerical, and political institutions. Cheapness of education, and a corresponding adaptation of time, are found indispensable to the general condition of society.

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* In the annual announcement, for 1842, of one of the principal medical schools in America, and associated with one of the oldest universities, and located in a populous town, after a statement of the average price of board, it is added that there exist "abundant facilities for those who may wish to board themselves."

The question is, therefore, settled on the immutable principles of truth—of a truth which is founded in the exigencies of our country. We have not the means, we have not the leisure, to follow the standard of European wealth; nor can we control our destinies by European legislation. He, who, in America, aims at the profession of medicine, with honors and dignities as inviting as in the aristocracies of Europe, but less seductive than the allurements of wealth, comes from a class where the blandishments of the latter have no existence. He has worked his way from elementary schools through the higher departments of academic learning, under the frigid discipline of poverty, and he enters our halls of medical education with little else than the hope that his career may not be arrested by insane exactions, now, for the first time, borrowed from the overgrown wealth of Europe, and her old and rich institutions.

Raise, therefore, beyond a certain limited poise, our standard of absolute requirements, and I repeat it, with no fear of contradiction, we shall turn from our medical schools most of their aspirants into more humble channels, or into the walks of empiricism. The exigencies of American physicians demand an early application to the business of life. If we would cultivate the field of medicine, we must look for an early harvest, or, my word for it, it will soon be overrun with weeds. But these necessities by no means preclude the highest advancement in medical attainments. When the harvest begins, then is the time for the most salutary stimulus of ambition; and whoever yields to its spur, will find abundant opportunity to carry his knowledge to the highest stretch of his intellectual constitution. He has but to remember that professional rank can only be acquired by various learning; and, in the language of Johnson, he "who proposes to grow eminent by learning, should carry in his mind at once the difficulty of excellence and the force of industry; and never forget that fame is not conferred but as the recompense of labor; and that labor, vigorously continued, has not often failed of its reward." * These indisputable precepts compel the aspirant at medical renown, or even at the highest practical success, to limit his enjoyments to a social intercourse with his patients, to a participation in the general enterprises of society, to the study of nature, and lastly, to all the literature which may elevate the mind. These should be his recreations as well as his employment; and as knowledge

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* Rambler, Vol. I.

flows in, an inexhaustible fountain of happiness is acquired, whose very spray clouds the gaieties of life, and renders the pleasures of the intellectual man as circumscribed as the voluptuousness of wealth is discursive in its search after sensual gratifications. One is a steady, refined, and unalloyed enjoyment; the other gross, discontented, and forever sated with every thing but the new. "Philosophical happiness," says Burke, "is to want little; vulgar happiness is to want much, and to enjoy much."

If we look for a *defect* in the intellectual culture of the graduates of our medical schools, we shall find a pervading one in the neglect of the opportunities which perpetually surround them,—in the apathy which they evince towards the literature of the profession. This, I am sensible, grows in part out of those exigencies which limit the academic and professional requisites that introduce the student to the honors of the profession, and which, in late years, have made greater demands upon his habits of economy, and have tended yet farther to cool the ardor of ambition. A sickly fashion has grown up of deriding elaborate works on science. This is especially true of the science of medicine, which, more than any other, demands critical and minute investigation. It is the fashion of the age to conceal or reject the past, in all its transcendent lustre. There is a general demand for the *multum in parvo*, for the latest fashion in the fewest words,—and the less of nature it embraces, the more does it receive the applause of the multitude. I will not say that we must go back to the ancients for the true principles of medicine, though we shall find the best of our own embodied in their writings. But I will say that they had better habits of observing nature than ourselves. These, we should study, and learn from them the true sources of our best conceptions of nature. The Greek and Roman philosophers surpass all succeeding generations in many of the most elevated occupations of mind. The best of our poets, sculptors, historians, orators, are only humble imitators of their excellence. They have not a rival, not an equal in our own times. And how truly are the principles of medicine, like those of the fine arts, engraven in bold relief upon the institutions of nature. If there be an apparent mystery, it is equally common to the whole; but it vanishes before the light of genius. The phenomena of disease were peculiarly attractive to such an order of mind; and springing then, as now, from the great laws of organization, the latter were grasped, as were the fundamental principles of taste, poetry, &c. The master-spirits of antiquity observed nature correctly, and drew their inductions from this only source of correct knowledge. They must therefore remain impregnable against all the adversities of time. (See my Med. and Physiol. Com. vol. 1, p. 333, vol. 2, p. 813.) A great work is to be done by the physician, and in proportion as deficiencies abound when he assumes the high responsibilities of his profession, it becomes him to investigate well every step of his experience, to employ his hours, or his moments of leisure, by day and by night, in garnering the experience which is supplied by the literature of the profession; and by rendering experimental observation and theoretical principles mutually subservient, he shall come, by degrees, to the attainment of distinguished worth, and the applause of mankind. It is true, as has been well said, that "men have sometimes appeared of such transcendent abilities, that their slightest and most cursory performances exceed all that labor or study can enable meaner intellects to compose; as there are regions of which the spontaneous products cannot be equalled, in other soils, by care and culture. But, it is no less dangerous for any man to place himself in this rank of understanding, and fancy he is born to be illustrious without labor, than to omit the care of husbandry, and expect from his ground the blossoms of Arabia."* "Every man," says the great moralist, "is obliged, by the Supreme Master of the Universe, to improve all the opportunities of good which are afforded him, and to keep in continual activity such as are bestowed upon him. But he has no reason to repine, though his abilities are small and his opportunities few. He that has improved the virtue or advanced the happiness of one fellow-creature, he that has ascertained a single moral proposition, or added one useful experiment to natural knowledge, may be contented with his own performance; and, with respect to mortals like himself, may demand, like Augustus, to be dismissed, at his departure, with applause."†

This is just in morals, as it is benevolent in principle, and where opportunities are limited, the doctrine should apply in its ample liberality. But it can scarcely serve as a mantle of charity where opportunities abound, as in the practice of medicine; though we may rest assured that the same great philosopher was universally right in affirming that "he who waits for an opportunity to do much at once, may breathe out his life in idle wishes, and regret, in the last hour, his useless intentions and barren zeal."‡

I have a few remaining words as to the nature of the admirable system of collegiate medical education which has spread over our land. It has grown out of the exigencies and enterprise of a young country and the republican character of its laws. It is adapted to the various conditions of society, and provides, in its lowest degrees, a class of physicians who are capable of yielding great benefits to the human family.

The medical students of the United States, who aspire at the honors of the profession, may be divided into three classes. One of these resort to the country schools, where lectures and board are low, though scarcely lower than in our cities. The advantage gained, therefore, consists in a more limited term of instruction, which diminishes expense, and often enables the graduate to enter sooner upon those practical labors which call loudly upon his attention. He goes forth, however, from these minor institutions with a fund of knowledge of immediate and salutary application, and which is capable of growing into a mine of inestimable value.

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* Rambler. † Idler. ‡ Idler

A second class are blessed with more ample means; and after husbanding their resources at one session of a country institution, they seek, at the next, the higher accomplishments which are to be found in the cities.

Lastly, come the aristocrats of our profession; made so through the difference of a few dollars, by those noble attainments which that little disparity in fortune qualifies them to bestow upon themselves. It is the aristocracy of mind and education—the only aristocracy which can advance, one iota, the dignity and the worth of man.

Here, then, we have a system of medical education remarkably adapted to the exigencies of our country; and for the very reason that it has sprung from those exigencies, under the stimulus of a general diffusion of knowledge, and the indomitable enterprise and ambition of our countrymen. Instead of deficiencies, it is only astonishing that a nation, just rising from its infancy, should have carried the general diffusion of knowledge far beyond any of the most enlightened states of Europe, should have reared more temples for the higher branches of education than can be found in all Europe together, should have its twenty-eight medical colleges, in most of which there are professors who do honor to the departments over which they preside. Wherever we turn ourselves, we are met by examples of aspirations at knowledge, whether private or public, which, however familiar, are so unusual in contrast with the infancy of nations,—aye, with the adolescence of the oldest communities,—that we often fail to regard the phenomenon as a reality of human enterprise. It meets us, indeed, in the forest as in places of greater wealth and refinement; and, even there, we behold institutions for the diffusion of medical knowledge commanding the attention of legislatures, and giving origin and impulse to various movements in science. Where, for example, was Missouri yesterday, and where is she to-day? "That State, which, within the memory of young men," says the able editor of the Boston Medical and Surgical Journal, (Aug. 3rd, 1842,) "was in the possession of savages, now vies with her sister States in the arts and refinements of modern society. Amongst the evidences of her onward progress, is the organization of a State Medical Society. The legislature has thus given a splendid exhibition of the wisdom of its councils, in chartering an institution which contemplates the preservation of the public health, almost in the commencement of its own existence;" the society having been incorporated in 1837. But, what is yet more to our purpose, the State of Missouri, in the year 1829, while yet a forest, where the white man mingled with the red, opened a university with a medical department of such commanding attitude that we can only look for its parallel to our own country.

Nor are there any institutions more important than the *country* medical schools, since these are adapted to the means of a large class of students, who, without them, would either make but humble attainments, or seek other channels for the business of life; whilst, as I have shown, the places of the latter would be supplied with jugglers and empirics. Nay, more, a large proportion of our second class, such as divide themselves between town and country, wanting the facilities of attending, at first, a minor course of lectures, and not having the ambition which is there inspired, would never seek the higher advantages of our cities but, in hopeless despair, would turn themselves to some other employment, or be compelled to dispense with the important advantages of public lectures.

The more, therefore, we consider this subject, the more we shall see the calamitous extent to which medical education would be reduced, or gross empiricism entailed upon society, were it not for our country medical schools; and it therefore follows, as a necessary axiom, that the standard of requirements cannot be increased in those institutions without equally defeating their objects.

It is therefore, also, as palpable as demonstration can make it, that our present system of collegiate medical instruction should remain without essential change in its *absolute requirements*, till such change can be sustained by a more general increase and diffusion of property, and by that diminished zeal for wealth which only follows its accumulation in the hands of many.

What is apparently lost in America by the more limited demands upon medical education than are exacted by a few European seminaries, is made up by the greater multiplication of our medical and academic institutions, by their general tone, and by the more general diffusion of medical knowledge. Nor is it a small part of our surrounding advantages, which go to the formation of character, of judgment, and practical habits, at the very outset of professional life, that our pecuniary wants turn us, almost from the cradle, to thoughtful habits of economy, and into the ways of industry. We thus, also, become early familiar with man in all his aspects; and with characters formed for decision by participating in the events of society, and early accustomed to mark the varieties of the moral constitution, we are better qualified, than they who spring from the nursery of wealth, to appreciate the various conditions of the physical, and to carry our habits of business into the chambers of the sick.

It is especially to moral influences proceeding from such causes, that I ascribe much of the acumen of American physicians in observing nature, and of concurring so remarkably in one decided practice—be it an active or an expectant plan which disease may demand; for decision and enlightened observation may be as necessary in one as in the other. American physicians are also generally familiar with the writings of some of those illustrious observers who form our best counsellors in the interpretation of nature; and it is only within a brief period that their attention has been at all directed to those oft-exploded physical theories of life which have so extensively supplanted the philosophy of organic nature in Europe. No obstacle, therefore, having been interposed to divert us from our habits of observing nature, and having no leisure for ambitious views, we have gone on with our former conceptions of the vital nature of disease, and of the operation of remedial agents, and would as soon wait for the plough to move itself in the furrow, as to adopt the imbecile treatment of that nation in Europe which studies the pathology and the diagnosis of disease only when its treatment has ceased to be possible, and which began the insane enterprise of reviving the worst absurdities of the darkest ages of our science. And how is it with our mother country ? Deeply tainted, yet struggling with others of transcendent worth in Middle Europe, to maintain the ascendancy of truth. But, how stands the descendant, in comparative practical habits ? The honest Britain still lives to witness the spirit of his declaration in greater force than ever, that, "after all British reproaches against the American government and American physicians, for permitting the yellow fever to commit such ravages, this yellow fever made incomparably greater proportional ravages in a British garrison, where the authority of medical police was unlimited." And yet again, says the same eminent man, "in the West Indies, on the continent of Europe, at home, wherever the British army was posted during the last war, its history of health has offered not only pretexts to a challenged rival to scorn the arrogant superiority of the British *modern* medical practice, but also reasons for an impartial man to pause upon the claim." *

Such were American physicians when in all America there were only two medical institutions, and when their exactions were only commensurate with the financial ability of the country. Well may we, therefore, spurn the imputations of practical inferiority which have been sometimes heaped upon us even by those who derive their principal title to consideration from being born in the midst of so much enterprise and worth. It is the jealousy of narrow ambition, or the envy of more selfish or of weaker minds. But, whilst I would denounce the spirit of innovation which would overthrow the best institutions of the land, I certainly agree that were a more extended and varied knowledge of true science within our early grasp, it would exalt us yet higher in the scale of philosophy, and in the practical pursuit of our calling. Nor will I leave this important subject without saying, that I am determined to be correctly understood and correctly represented upon one fundamental point, at least—and that is, that I am an ardent advocate of a profound medical education—ever have been, and so mean ever to remain. I am ready to lend all my feeble aid to any plan which may be regarded by judicious and disinterested men as a feasible improvement of our present collegiate system.

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* Beddoes on Fever and Inflammation, p. 235.

A DEFENCE

OF AN INTRODUCTORY LECTURE ON THE

"IMPROVEMENT OF MEDICAL EDUCATION IN THE UNITED STATES,"

AGAINST AN ATTACK BY THE

Medico-Chirurgical Review.

BY MARTYN PAINE, A.M. M.D.

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DEFENCE OF AN INTRODUCTORY LECTURE.

To the Editor of the Boston Medical and Surgical Journal.

Sir,—I see by the April No. of the Medico-Chirurgical Review that that Journal, after being long passive under my "Examination of Reviews," has ventured upon a misrepresentation of the objects of my humble Introductory Lecture "On the Improvement of Medical Education in the United States," delivered before my Medical Class at the session of 1842–3.

The Journal is pleased to complain, in the first instance, of my efforts to counteract the wretched speculations by which Liebig, and such followers of his as the Medico-Chirurgical, are attempting to overthrow the great fabric of medicine. The Journal is quite welcome to its lament over my successful efforts in that particular; and, having accomplished the task which I had prescribed for myself, I shall pass on to the Reviewer's misrepresentation of the main object of my lecture.

The Reviewer remarks that the lecturer proceeds to consider the "'*Improvement of Medical Education*,' by which, however, we very soon found, to our *utter astonishment*, the learned lecturer means neither more nor less than *lowering* the standard of professional requirements."

Whoever may have read my "*Examination of Reviews*" will not be surprised to learn that the foregoing statement is *utterly false*, and that the objects of the lecture are exactly the reverse. The whole article, turning upon that misstatement, is equally a tissue of misrepresentations. It was, however, a fitting occasion to strike a blow at all my former efforts, by representing me in the attitude of discouraging those profound attainments in medicine for which I have so long and laboriously contended. And yet who, for a moment, can believe that one, like myself, who has devoted his life to the cultivation, if not to the improvement, of medicine, should, after a life of such unintermitting toil, have become, at last, recreant to the great cause for which he has been thus long an unflinching champion?

The Reviewer is also false in attempting to convey the belief that my remarks on the "Improvement of Medical Education" were designed to be of universal application; whereas they were wholly and expressly limited to the existing state of society in the United States; nor is there a word said by the Reviewer of a main intention of the lecturer to sustain the medical colleges in the interior of this country.

No one laments more than myself the difficulties which render impracticable in this country, at the present time, the high professional requirements which are so noble in some of the Continental schools of Europe. Nevertheless, I have no objection to a comparison between the practical habits of British and American physicians, and it may be a profitable lesson to the Reviewer if he will advert to the sentiments of his own distinguished countrymen upon that subject as set forth in my *Medical and Physiological Commentaries*, Vol. 2, p. 664—675. And, when I regard the satirical manner in which the Reviewer is pleased to indulge his humor towards the whole profession in the United States, I might, in a spirit of resentment, refer him to many late Nos. of the *London Lancet* for vivid descriptions of the existing state of medicine in Great Britain; but this is neither conformable to my taste, nor would it comport with my sense of justice towards a large body of British medical philosophers. And yet, should I not offer an example of the general bearing of this periodical towards the medical literature of a country whose imperishable glory is largely connected with that literature, and which has so recently derived fresh lustre from many brilliant gems of the "sea-girt isle," it might be imagined that my reference to the London Lancet is without foundation. Let us, therefore, have an example from that far-famed periodical of its late patriotic exhibition of British medical literature. Thus, then, the *Lancet*:—

"First," says its veteran editor, "with respect to works on pathology and the practice of medicine. If we seek among English writings on these subjects for works like those of Andral and Louis, Chomel and Piorry, and a host of other distinguished French practitioners, we shall assuredly seek in vain, finding little that is for a moment comparable with them, either in originality of observation, or richness of materials." Again; "look, for example, at the state of British physiology. Of what does the great majority of our books on this subject consist? Of compilations; of old views cooked up as new discoveries; of annotated translations; or, at best, of able and comprehensive digests of materials that were already before the public in other forms. Compared with the profound and original works which are continually issuing from the German press, how humiliating a contrast is formed by English medical literature."—*London Lancet*, May 6, 1843.

But my present business is with the Medico-Chirurgical Review, which, on a former occasion, I convicted of a coward's falsehood; for, unlike its associate (the British and Foreign Medical Review) in a crusade against my literary labors, it had not the spirit even to stand by its own corruption, but received, with tacit submission, the brand of infamy. Nor shall I be now restrained from again indicating, by the Reviewer's own admissions, the spirit which has hitherto prompted his misrepresentations of the labors of American physicians, and his vituperation of the American medical profession. In the article with which my unpretending lecture has been honored is a reference to a criticism on Liebig's *Animal Chemistry* in the North American Review, which has been so perverted by the imagination of the foreign Reviewer, that he is thrown into a paroxysm of indignation which demanded a nation's atonement. We read, for example, that,—

"With respect to Dr. Paine's learned coadjutor in the Quixotic attempt to demolish Liebig's theories, whose tirade appeared, if we mistake not, in the North American Review for October, 1842, we shall only say, that its *vulgar verbosity, unbecoming personalities, and rancorous hatred of everything British*, render it perfectly safe from our notice; *non tali auxilio, nec defensoribus istis tempus eget.*"—*Medico-Chirurgical Review.*

Although there is *nothing* of the foregoing nature in the Review which has thus excited the displeasure of our critic, it nevertheless suited his purpose to have it so. And, if the reader will now carry his eye two pages farther on, in the Medico-Chirurgical, he may possibly learn that the Reviewer considered some pretext important to a justification of his own malevolence towards a land from which he has long derived a liberal, a generous patronage. We there find, for example, that,—

"We cannot but remark with satisfaction the fortunate condition of the University of *Pennsylvania.* The '*drab-colored men*' may well make doctors of their sons, seeing that they can pay the *matriculation fee* with other people's money. We only trust that that very fee is not *repudiated*, and that in that very respectable State there is in their dealings with each other *honor among thieves.*"

Perhaps I may now respectfully ask the Reviewer what is his opinion between a fabrication which imputes to the North American Review "vulgar verbosity, unbecoming personalities, and rancorous hatred of everything British," and the actual vituperation which is perpetrated by the same false accuser towards an honorable profession in the United States?

Here I stop; not doubting that the impartial reader will trust my affirmation that I remain, as ever, a devoted advocate of the highest culture of medical science; that my humble lecture has exclusively and ardently for its objects the interests and dissemination of that science; and, finally, may I not express the hope that I may be permitted to go on hereafter in the promotion of those objects without farther molestation, and especially without a rancorous falsification of my arduous labors in behalf of medicine. I remain, Mr. Editor, very respectfully yours,

New York, May 9, 1844. MARTYN PAINE.

SUPPLEMENT SECOND TO THE HUMORAL PATHOLOGY.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—Dr. Carpenter, in his review of the "Medical and Physiological Commentaries," as contained in the April No. (1841) of the British and Foreign Medical Review, and a Reviewer of the same work *(by a singular coincidence)*, in the April No. of the Medico-Chirurgical Review, are pleased to deny the validity of my Essay on the Humoral Pathology, but do not attempt its refutation by a single fact or argument, nor have I anywhere seen any demonstration against it, beyond the *ipse dixit* of the writer. The Reviewer, in the Medico-Chirurgical, remarks that—

"The question at issue, as Dr. Paine *fairly states it*, is this—'Whether foreign morbific causes and remedial agents, in their ordinary modes of operation, produce their primary effects upon the solids or upon the blood, and the latter become the cause of disease in the former; whether we have hereditary humors, as gout, scrofula,' &c. &c. We are satisfied with the answer of M. Andral to this question," &c. &c.—*(Rev.* p. 400.)

Now, M. Andral unequivocally affirms, and attempts to demonstrate by syllogistic reasoning, that ALL diseases are owing to a *primary vitiated* state of the blood, and that there is but one therapeutical principle, which is founded upon this pathological dogma, and which consists in cleansing or otherwise restoring the blood by the direct action of remedial agents, without any reference to the state of the solids. This I have not only abundantly shown by quotations from M. Andral (*Comm.*, Vol. I., pp. 415, 618, 630—631, 636—637, &c.), but that it is the doctrine of many other living and distinguished medical writers. (See *Comm.*, Vol. I., pp. 392—400, 417, 419, 433—435, 447, 464—465, 488, 517, 540 *note*, 541, 543, 531, 533—539, 565, 576—581, 582, 586, 587 and 608 *notes*, 609, 611, 613, 639—640, 663, 667, 674, 696—698, &c.) Dr. Latham states the whole pathological and therapeutical doctrine, when he says that we shall doubtless find the seminal principle of disease in a pravity of the blood itself, and that the worst forms of fever will yet be cured by table-salt, in virtue of its direct blood-making faculty.*—*(Comm.*, Vol. I., pp. 397, 657.) Other late and distinguished writers are quoted to the same effect. The British and Foreign Medical Review, and the Medico-Chirurgical Review, maintain in many of their late articles the foregoing doctrine, or do, at best, but tolerate solidism and vitalism where it would be manifestly preposterous to adopt the humoral *rationale*.—(See *Comm.*, Vol. I., pp. 392—400, 534, &c.)

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* See Latham's Lectures on Clinical Medicine, p. 53. 1837. American edition.

I come now to the specific object of this communication, which is to substantiate the concluding sentence of the following quotation from the "Commentaries," and with which the Essay on the Humoral Pathology commences. Thus:—

"Having hitherto investigated the character of the forces and actions of life, we are better prepared for considering the important subject of the Humoral Pathology.

"What recollections are not inspired by our introductory sentence! What mind so insensible to the past, that it has not already travelled over the various eras of medicine, and passed in review those countless sages that gave distinction to each? Who has not traced from Galen to the last of his race, the brilliant achievements, the heroic renown, the unexampled career of humoralism? Who has not fancied that *last* man standing in solitary, hopeless defence, like Caius Marius swearing revenge over the ruins of Carthage? Dividing into adverse schools, they yet maintained a common bond of union *through the doctrine* which is now uniting us with remote ages, *and with every empyric in the land*."—(*Comm.*, Vol. I., p. 385.)

I shall subjoin an advertisement by the acknowledged chief of empyrics, which, it will be seen, embraces the whole philosophy of medicine as now cultivated by distinguished humoralists, reduces pathology and therapeutics to a simple "*Unit*," strips them of all relation to physiology, and in the candid language of Magendie—"all the physician can do is to order certain remedies, which, if necessary, *the nurse could prescribe equally well*." "You saw me," he continues, "give rise, at *my pleasure*, to pneumonia, *scurvy*, *yellow fever*, &c., not to mention several other affections which, so to speak, I called into being before you." And all this upon cats and dogs.—(*Comm.*, Vol. I., p. 397.) It is said that Professor Chapman is wont to ask his medical class whether they ever saw a hen with smallpox. Of *vitality*, Magendie speaks also the common doctrine of the physical and chemical philosophers of life. "For my part," he says, "I declare boldly that I look upon these ideas about vitality, and the rest of it, as nothing more than a cloak for ignorance and laziness."—(*Ibid.*) This degeneracy of medicine has grown out of the recent efforts to construe the results of life by the forces of inorganic matter and upon physical and chemical principles.

Endless experiments have been going on in all parts of Europe with injections of putrid and other morbific substances, and even of quicksilver, into the circulation, to extort from their results the conclusion that the ordinary causes of disease are *absorbed* and *vitiate* the blood. These experiments have become the grand foundation of the doctrine, whose practical consequences are so forcibly and so justly set forth by Magendie. Or, if we turn to the metropolis of Great Britain, where Hunter and his compeers exalted medicine to the highest dignity amongst the sciences, we shall have some idea of the *intellectual* respect in which it is regarded by the ablest champions of humoralism, by quoting the precepts lately inculcated by Dr. Latham upon a class of medical students.

"Knowledge," says Dr. Latham, "may be an encumbrance as well as a help." "I am acquainted with men who never have done, and never can do, anything, *because they know too much*." "Nothing is more common than to hear it said of some *eminent* and *distinguished* person—'Eminent and distinguished as he is, what would he not have been had he possessed the learning of such an one?' Whereas, if he had possessed one particle more of learning than he has, he would have been nothing at all; it would have weighed him down and he would never have been heard of"! "Many a clever man practises physic with tolerable success, who has never troubled his head about morbid processes, and who has not the remotest notion how those things come to pass which he has been witnessing, in their effects or their symptoms, all the days of his life"!—*(Latham's Clinical Lectures*, pp. 16, 59.)

On the other hand, we are told by Hippocrates, that medicine is related to all other sciences, that its philosophical attainment, and its practical application, are the most difficult of human pursuits, that a "philosophical physician is like a god," and he urges upon his son the study of mathematics as an important foundation for medical inquiries. Zimmermann and other illustrious philosophers, after the experience of more than two thousand years from the precepts of Hippocrates, maintain that it is only men of genius who can, without erudition, grasp the principles of the healing art.—*(Zimm. on Experience in Medicine.)* Let us not, then, throw discouragements in the way of intellectual culture, where it is so much demanded by the highest interests of humanity.

I come now to the demonstration that the whole science of medicine, as cultivated by the most able humoralists, is lucidly expounded, and insusceptible of improvement or further illustration, in an *advertisement* by BRANDRETH, which I copy from a newspaper of the 23d of August, 1841; and I shall also feel it due to the cause which I advocate, and for which I have been wholly misrepresented and calumniated by the two leading medical Reviews of Europe, to incorporate this communication with the "Medical and Physiological Commentaries," along with my pamphlet.

Advertisement by Benjamin Brandreth.—"DISEASE A UNIT. *Impurity of Blood the only Disease.* How simple, yet how wise, how good and beautiful are all the laws of Nature! Simplicity and truth are stamped upon every law of the creation. The mighty worlds which roll in space, in every degree of velocity and direction, are all governed by ATTRACTION OF MATTER TO MATTER. This principle governs the human body. *Brandreth's Vegetable Universal Pills* attract all impurities of the blood to the bowels, which organ expels them from the body. Attraction and disease are both UNITS. All accidents, or infections, only affect the body in proportion as they occasion *impurity* of the blood. The bowels, for instance, are costive—this most important organ is closed—the consequence is a great accumulation of impurities, which, as they cannot get out by their usual passage, are *forced into the blood*. [And so say the humoralists of accumulated urine.*] Thus fevers, colics, rheumatism, coughs and colds, are often produced. But, let *Brandreth's pills* be used in such doses as will effectually evacuate the bowels, and health is restored at once. *Hot weather*, by occasioning *debility*, produces *impurities of the blood;* from which arise dysentery, cholera morbus, cramps in the bowels, feebleness, pains in the back and hip-joints, headaches, &c. &c. These unpleasant companies are speedily removed by a few doses of the *Brandreth pills*, which soon restore health *by purifying the blood.* Grief, great anxiety of mind, much watching, fear, bad food, intemperance, residence near marshy land, tend, in a powerful degree, to promote *impurity of the blood*, which soon shows itself in erysipelas, consumption, epileptic fits, apoplexy, scurvy, fever and ague, derangements of the stomach and bowels, all which symptoms will soon be removed *by purifying the blood with the Brandreth pills.* [See Andral in "*Comm.*," Vol. I. ut. cit.] Smallpox, scarlet fever, putrid fevers, even spotted fever, and fevers of all kinds, are propagated only by those whose blood is in a state of *impurity.* These maladies are mild or virulent according as the *blood* be charged with *impurities* previous to the infection being received, and never attack those whose blood is in a state of purity. [See *Comm.*, ut cit.] The *Brandreth pills*, *by purifying the blood*, soon cure these maladies. Ulcers are produced by *impurities of the blood.* The part where it breaks out has, in days gone by, been injured, and therefore its powers *could not repel* the impurity of the blood when it settled upon it. [See *Comm.*, ut cit.] Soon the acidity excoriates and opens the ulcer. Here we have a *drain* or outlet opened for the *bad humors*, for the impurity of the blood to pour out of the body. [See *Comm.*, ut cit.] *Brandreth's pills*," &c. &c.

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* See Comm., Vol. I., p. 601—608, &c. Were urine absorbed, it would produce violent inflammation not only of the absorbents of the bladder, but in all other parts. And so of bile.

Such, then, is a truly luminous exposition of the whole doctrine of humoralism—only Brandreth's philosophy does not recognize the *absorption of his pills.* He seems to be sensible that they are incapable of converting diseased and impure to healthy blood by mixing with that fluid, and has therefore substituted the ingenious hypothesis of attraction—for which he finds an analogy in cohesion and gravitation, just as Dr. Carpenter does in the "development of the magnetic powers of iron" for the "development of the vital properties which are dormant in the elements of matter," when the former, like the latter in respect to organization, is placed in a favorable relation to the magnetic influence.

How far it is probable Dr. Carpenter may be indebted to Brandreth's advertisements for his conceptions of the humoral pathology, as set forth in his review of the "Medical and Physiological Commentaries," can only be inferred from the general conviction of plagiarism which I have proved upon him in my pamphlet, and especially his signal preference of the writings of the Rev. Dr. Channing, as exhibited in his "Principles of General and Comparative Physiology," and in extensive Essays in the British and Foreign Medical Review. It is difficult, however, to conceal the gratification at finding American authors in such high favor with our distinguished transatlantic friends; and, humble as may be our American humoralist in the estimation of the eminent philosophers of his school, it cannot be concealed that a more able exposition of the doctrines of humoralism has never been vouchsafed to the world, nor the whole philosophy of medicine more comprehensively set forth. This I am induced to claim in behalf of America.

Beyond the haunts of the empyric, however, let me once more say, that America has scarcely yet been tainted with the physical doctrines of life or their natural offspring, the vagaries of humoralism. She still presents, in the midst of nations whose eminent men have, at former times, directed the destinies of science, the astonishing spectacle of an almost entire profession devoted to the Hippocratic method of observing nature, clinging to the experience of the past, avoiding the fruits of that philosophy which is founded on the ruins of nature, nor yet seeks an interpretation of her vital phenomena in the crucible of the chemist or through the glass of the optician. She remains unshaken by the convulsion around her.

Let us continue to cultivate physiology as the most profound and the sublimest department of nature—to look upon the invasions of physics and chemistry as the ambitious strides of a giant who would gain a monopoly of the earth, and upon any act of submission as a degradation of ourselves and of that philosophy which can alone protect the Attributes of Him who gave it existence. Let it still be the ruling genius of this land to consult the understanding first, and the imagination and senses next; and, whatever we may obtain from abroad, that concerns the interests of medicine, may it still be subjected to rigorous analysis, and to the test of reason and individual experience. Let us still go on as we have been going, "gathering like the bee from abroad, but digesting that which is gathered *by his own virtues*;" still leaving the phantom of "vitality and spirituality in the elements of matter"—the "frail embryo of organic chemistry"—the dependents upon morbid anatomy for the diagnosis of that disease which has put an end to its speculative treatment—homœopathy—animal magnetism—"artificial digestion"—the mathematical humbug—still leaving these, and all others like them, "to spin out, like the spider, all their own bowels," and thanking "the empyrical philosophers, who, like pismires, only lay up and use their own store," for any proportion of their harvest they may be willing so spare—being ever willing to receive from the foregoing phalanx their castigations for our unambitious perseverance upon the well-beaten path of nature, or their contumely for our undaunted energy in the treatment of disease, or their maledictions for waiting upon nature when art has won its triumph.

Respectfully yours,

New York, August 23, 1841.

M. PAINE.

ERRATA.

In the pamphlet entitled "*Notice of Reviews*," &c., republished from the Boston Journal, at page 3, line 18, for complete read *complex* ; line 40, for Platonic read *Plutonic;* line 48, for constituted read *constitutes.* Page 7, line 32, for miracles read *miracle.*

THE

CHOLERA ASPHYXIA

OF NEW-YORK.

LETTERS

ON THE

CHOLERA ASPHYXIA,

AS IT HAS APPEARED

IN THE CITY OF NEW-YORK:

ADDRESSED TO JOHN C. WARREN, M. D., OF BOSTON, AND ORIGINALLY PUBLISHED IN THAT CITY.

TOGETHER WITH

OTHER LETTERS, NOT BEFORE PUBLISHED.

BY MARTYN PAINE, M. D.

New-York:
PUBLISHED BY COLLINS & HANNAY.
Clayton & Van Norden, Printers.

1832.

Entered according to the Act of Congress, in the year one thousand eight hundred and thirty-two, by Collins & Hannay, in the Clerk's Office of the District Court of the United States, for the Southern District of New-York.

TO THE READER.

When I addressed the first of the following Letters to the distinguished gentleman with whom I prepared for my profession, I was influenced principally by the desire, so natural and common to pupils, to repay by every means in their power, and if possible, however slightly, in kind, the unlimited debt which they owe to their instructers. The early appearance of the Cholera in New-York, a disease before unknown in our country, afforded an opportunity for the humblest among us, to communicate information which might be interesting, and perhaps useful, to the most skilful and learned and eminent in other cities. And I could not but apprehend, that this inscrutable and relentless stranger would, in his arbitrary and fearful progress, visit Boston, where I have many friendships and near attachments. The little therefore that I could, by diligent observation, learn of the Cholera, I was irresistibly impelled to communicate to one to whom all in that city look for professional aid or instruction. But I had not the most distant expectation, that my letters would meet the public eye, nor would my own judgment have led to their publication. I will not, however, deny the gratification I felt, at learning that their publication had been deemed useful by one, whose approbation I have ever wished to deserve. The first four letters appeared originally in the Boston Daily Advertiser and Patriot, and in the Boston Weekly Messenger and Massachusetts Journal, and it was not until after they had been published in those papers, that the idea was suggested to me of offering them to the public in their present form. As soon as I had adopted this suggestion, I thought it would be better to reserve the remainder of the letters, until the whole should be published together, and the fifth and sixth letters are accordingly now for the first time presented to the public.

I am sensible that these letters can possess no other value than such as they derive from my means of observation, and the diligence with which they have been improved. I ought, therefore, to say, that from the first appearance of the disease in this city I have sought it at all times and in all places, and indeed much more constantly than was consistent with my health, and ordinary engagements. In connexion with two medical friends, I made, on the second of July, a minute post mortem examination of two of the first who fell victims to the disease, and which were the two first dissections made in the United States. These examinations are fully described in the seventh letter, which was originally published in the New-England Medical and Surgical Journal. Since then, besides my own private practice, I have daily visited as many of the hospitals as my engagements would admit of, and have made and witnessed numerous dissections. To have described these various post mortem examinations would have been an unnecessary repetition of similar appearances. I have thought it worth while to preserve only such cases as would exhibit the several varieties of symptoms which have passed under my observation. These will be found in the eighth, ninth and tenth letters, which have also been published in the Medical Journal. One of these also announces the important discovery by Dr. Gale, a scientific chemist of this city, of oil existing in a free state in the blood of Cholera subjects.

The letters, except those describing the dissections, are printed in the order in which they were written. The parts of the subject of which they treat cannot, therefore, be expected to be formally or methodically arranged. Each letter was however designed to embrace a single topic. The first presents a general view of the disease; the second relates to the nature and treatment of premonitory symptoms; the third to the importance of attention to food, as a means of prevention; the fourth to the treatment of Asphyxiated Cholera; the fifth to the nature and philosophy of the disease; and the sixth gives a circumstantial account of its symptoms as they appeared in this city. The other letters contain the dissections.

I trust I need not apologize for those occasional repetitions and other faults, which are almost incident to letter writing. But I cannot but regret, that the more impaired condition of my health has unfitted me for the task of completing the letters as I wished to have done, and forbade any thing like the revisal and correction which they required. Less than this I could not, and more I need not say, to a candid public.

NEW-YORK, *September 14th*, 1832.

LETTER I.

New-York, *August* 10*th*, 1832.

Dear Sir,

I had intended to have earlier acknowledged your very flattering attentions, and to have expressed the interest and instruction with which I perused the Report of the Massachusetts Medical Society; which, although purporting to express the opinions of others, is undoubtedly one of the best original essays which has appeared on the subject of Cholera Asphyxia. This delay, however, enables me to add a few words in relation to the disease as it has appeared in this city, which I do, not from their novelty, but to show you how Nature and Art have still travelled the beaten path.

The Cholera Asphyxia has presented to our observation the same combination of phenomena that has distinguished the Epidemic in other quarters of the globe; and when the varieties of constitutions, the differences in habits, sex, age, &c. are duly considered, the symptoms are remarkably uniform, particularly after asphyxia has supervened. The disease has been less malignant here than at Montreal and at Paris, which is probably to be ascribed to the better ventilation and greater cleanliness of our city; for your own observation will yet assure you, that the disease, at least in cities, requires for its production the co-operation of local miasmata with the general epidemic poison; and doubtless the same local developements give intensity to the poison wherever it prevails. Well favoured cities, therefore, with spacious streets, and especially where a proper attention is paid to the cleanliness of yards, houses, &c., and in which a crowded population is not permitted during the visitation of the Epidemic, may expect to reap a rich reward from measures that are so clearly dictated by experience and philosophy. In New-York we may be said rather to have suffered several endemics than a general epidemic. The epidemic poison has not been sufficiently active to produce a full predisposition to the disease, without the co-operation of local causes. It has been confined to the malarious part of the town, and generally to such houses as are small and crowded with inhabitants,—in some instances not less than a dozen in one house having died in rapid succession.

Although we may recognise in a very striking degree the phenomena of the Asiatic disease, they are more or less distinct in different subjects; each case, however, presenting nearly the same combination. The symptoms most liable to modification, are lividness and collapse of the features, these being more frequently absent than has been remarked in other countries. Asphyxia, or nearly so, is a uniform symptom of the stage of collapse; so also are the unequal distribution of heat, vomiting and purging,—almost invariably of a fluid resembling rice water,—very generally spasms, cold sweat, corrugated skin and blueness of the fingers, cold tongue, cold breath, suppression of the glandular secretions, &c.

The morbid appearances on dissection have been less uniform than the symptoms, and our pathology has been consequently much embarrassed. The small intestines commonly exhibit some appearances of congestion either of the mucous or serous coat, generally of the former; the large intestines commonly blanched, especially if the diarrhœa has been profuse; the stomach nearly natural; liver sometimes greatly injected, but often very natural; kidneys, spleen, and pancreas natural; bladder empty and generally contracted. The gall bladder contains from one to two ounces of black, green or yellow bile. There is generally, though not always, an absence of this fluid in the intestines, which usually contain a substance similar to that evacuated during life. It has been well compared to rice water, with flocculent matter, which is soon deposited. The texture of the small intestines, particularly, is often softened, which has induced some physicians to ascribe this result to a violent inflammation of these organs, and to regard this action as the proximate cause of the disease. But so far as symptoms and appearances, that usually denote inflammation, are necessary to this theory, they are certainly nearly wanting in Cholera Asphyxia,—venous congestion being almost the only sensible condition of the blood-vessels. Both cavities of the heart are sometimes filled with black blood, and they are sometimes empty, —appearances which also attend the lungs. The brain is more uniformly injected than any other organ, and usually contains, either in the ventricles, or within its membranes, a preternatural quantity of serum; yet the integrity of the mind is not much impaired. The veins of the spinal sheath are also commonly full.

Here, as every where else, the disease was most malignant at its irruption, and has become milder as the epidemic air has abated. Most of the subjects perished, at first; and our greater success, now, arises from the milder condition of the disease, from a longer duration of the premonitory symptoms, and from the more susceptible class having been swept away. The nature and treatment of the epidemic are probably no better understood than when it first invaded Europe. The Asiatic physicians appear to have exhausted the subject, and the more we have departed from their modes of treatment, the less successful have been the results. It is certain that the disease was less fatal in Asia, where it is supposed to have been most malignant; and it has been destructive in an increasing ratio ever since it appeared in Europe. At Quebec and at Montreal scarcely any recover, at this late period, after the accession of Asphyxia. Here the disease having been milder, the recoveries, after that stage, have probably been about one in six. If the habits of the patient have been regular and temperate, perhaps it is one to four or five that he will recover, if asphyxia have not existed more than one hour; but no such good fortune awaits the intemperate. The noise which he hears is his funeral knell.

I have not remarked that changes of temperature or atmospheric phenomena, have exerted much influence on the character of the epidemic. It seems to have been governed by its peculiar laws, which have been scarcely modified by accident. The irruption took place when the weather was delightfully serene, and the city most remarkably exempt from disease. Diarrhœa had not even preceded it; and it was an astonishing spectacle to witness the work of the destroyer without the usual admonitions of his approach. It was then that we had *Death* and not *disease* in our city. His march was rapid, under a cloudless sky and a most genial temperature; and the mortality acquired its maximum during this seemingly auspicious state of the atmosphere. There then began a decreasing ratio, the weather and temperature becoming more variable, and the present storm, which has continued a week, seems not to have retarded its decline in the city; while in the neighbouring country the disease is appearing most malignantly under the same conditions of the atmosphere.

What are the exciting causes? It has been very rare, if it has ever happened in this city, that the epidemic poison has done more than predispose the system. Other causes have been necessary to the developement of the disease, and perhaps none have acted with greater intensity than the exhalations which arise from filth and decaying materials. Indeed, the agency of these causes appears to have been almost indispensable, for few have suffered the disease who have lived temperately and prudently without the malarious districts. Consequently the disease has chiefly prevailed, and deaths have principally occurred, among the most abject of the lower orders,—attacking constitutions that have been rendered susceptible by vicious habits or broken down by disease. Still we have lost some valuable citizens, whose habits were temperate, and whose lives were exemplary; but let us convert their loss to at least one good purpose; let us avoid those exciting causes which, in almost every instance, have sent them prematurely to the grave. These causes have generally consisted in some kinds of indigestible food; what in ordinary conditions of the atmosphere are inoffensive, but which are death to a subject predisposed to cholera. Garden vegetables, and almost every species of fruit, have been known to develope the disease; and the prudent part of our population now restrict themselves to lean fresh meat, potatoes, boiled rice, stale bread, eggs, milk, butter, tea and coffee, and it is considered hazardous to extend the variety. Fatigue, imperfect clothing, currents of air, exposure to the rain, and depressing passions of the mind, have been known to create the cholera.

We have not been able to add any thing new to what has been long known, or rather unknown, respecting the proximate cause of the malady. Most of us are well satisfied that it possesses nothing in common with the ordinary cholera morbus, but that the diseases are essentially distinct. The hypotheses are very various; some regarding it as a local affection, and others as constitutional; some referring it to a primary inflammation, congestion, or some unknown modification of the action of the alimentary canal; others to congestion of the brain, or of the spinal marrow and ganglia; while others look alone at the diminished glandular secretions,—and still another places the whole mischief in the carbonization of the blood,—and yet another regards the absence of the saline principles as the *sine qua non*. To me, however, the disease is just as obviously a constitutional one. We have frequently witnessed the invasion of asphyxia, without any premonitory symptoms, when it has been sufficiently apparent that all the functions and all the powers have been simultaneously affected. If this hypothesis be true, we shall understand why one important organ is at one time apparently sustaining much of the burthen of morbid action, and at another none at all. From so great a diversity of opinion respecting the proximate cause of cholera there would necessarily result a great diversity of treatment; but the variety of means employed very evidently arises, in part, from the want of success which attends every method. We have no difficulty with the disease in its preliminary stages; but after asphyxia has fairly supervened, I do not believe that one patient in six is restored by any process, whatever may have been asserted on this subject. I have seen much of the disease, and have looked at it impartially; and I express a solemn conviction on the subject of its fatality. I have seen every method tried, and in some I have no confidence at all. I have none in what has been denominated the *camphor treatment*, exclusively; none in the *exclusive* use of ice internally. I have seen the best results from powerful external stimulants;—calomel more or less modified by small quantities of opium, according to the irritability of the mucous membrane, and camphor is a useful addition if spasms be present; friction with camphor, or applications of ice for the relief of spasm; stimulants moderately to sustain the sinking powers, of which ammonia seems the best. The irritability of the mucous membrane is easily allayed, and is very rarely an obstinate symptom,—the most prominent, during the stage of asphyxia, being the absence of pulse, unequal distribution of heat, restlessness and altered expression of countenance. We have nearly abandoned transfusion, from its almost uniform failure. The recoveries I do not think have been in the ratio of two to fifty. The temporary effect is very encouraging, but the patients soon die. Our best practitioners employ calomel in large and small doses, some preferring 15 to 25 grains once in three or four hours, and others five grains every hour. I have seen no good effect from blood-letting in any instance after asphyxia; but this may have arisen from the quantity having been small, and the difficulty with which it is invariably abstracted. From the pathology of the disease, and the experience of the Asiatic physicians, I have been in favour of the remedy.

I regret that my time will not permit me to add more at present. Your citizens ought to be well informed of the fatality of the disease after the stage of asphyxia, and of its mildness before that stage supervenes, that they may be early induced to attend to the premonitory symptoms.

Very respectfully, &c.

LETTER II.

New-York, *August* 18*th*, 1832.

Dear Sir,

I had yesterday the pleasure of receiving your favour of the 15th inst., and I was certainly agreeably surprised, that my remarks on the subject of Cholera Asphyxia should have afforded you the interest you do me the honour to express, and more particularly that you considered them worthy of publication. In relation to your inquiries respecting the symptoms which have marked the accession of the Cholera in this city, there exists here, as in other countries, the remarkable fact, that the earliest manifestations of disease are almost uniformly mild, and bear no corresponding ratio to the tremendous developement which suddenly succeeds after a certain duration of that premonitory stage. This introductory stage is very rarely wanting, and its duration and symptoms depend much on temperament and habits. The symptoms generally denote some impaired function of the digestive organs, and usually consist of diarrhœa, frequently connected with nausea and vomiting. This condition in many instances is preceded by constipation, which, however, is not so uniform as has been imagined, but which, if neglected, is very liable to result in diarrhœa. There is, indeed, a general tendency to looseness of the bowels, and they, who have been habitually compelled to employ some laxative medicine, find now an ample substitute in the epidemic influence. This is a fact, which, I think, cannot be too well understood ; for it is this impaired state of the digestive organs, this tendency in all to looseness and diarrhœa, which have rendered our precautions in relation to food so indispensable, and which, when disregarded, have taught a lesson of prudence that few would have heeded, but for the terrible penalty so evidently connected with the indulgence. If the habits of the patient have been prudent, especially in respect to food, the diarrhœa is moderate, continuing, if it terminate in malignant cholera, for many days without vomiting, and not productive of much prostration or distress, till collapse suddenly supervenes. If indigestible food be the exciting cause in a temperate subject strongly predisposed, vomiting is an early symptom ; which, however subsides, and there only remains for a while, the diarrhœa, afterwards maintained by slighter errors in diet, till it sooner results in Asphyxia than in more abstemious subjects. I have not observed that constitutions impaired by disease, where great prudence has been practised, are more obnoxious to cholera, or that collapse sooner succeeds the premonitory stage than in the robust. Particular circumstances have led me to attend closely to this fact, and my observations on this subject do not concur with book authority. But it is equally true that constitutions, impaired by intemperance and prostitution, are peculiarly the subjects of this pestilence; and I cannot agree with Mr. Fife of Gateshead, "that drunkenness is not a powerful predisposing cause," though doubtless very much is to be ascribed to the influence of other accidental causes,—particularly excesses in food, and the filth in which they riot. The succession of primary symptoms is more rapid in such instances; but still there is commonly a period for the successful intervention of art, but which is generally neglected. The aged sink quickly under the diarrhœa, if the choleric predisposition exist; and children, though not often attacked, pass rapidly through the several stages of the disease to a fatal termination. Diarrhœa and vomiting do not always distinguish the premonitory stage; but it is sometimes denoted only by headach, loss of appetite, oppression at the chest, &c. and again spasms are known to have been the earliest symptom, and at first the only prominent one, and susceptible of relief. During the existence of diarrhœa, the appetite often remains unimpaired, and I have frequently known it to have been morbidly increased just before the occurrence of that symptom, and the patients have sunken rapidly into an incurable asphyxia. Something, no doubt, is to be ascribed in such instances, in explaining the accelerated march of the disease, to the morbid indulgence.

Much may be said on this very interesting subject of premonitory symptoms, on which are suspended the life of the patient and the happiness of friends, and which may determine the fate of both, as they may be duly noticed or disregarded. It is certain, however, that it is not one of the least attendant phenomena, that the patient, whose fears had been before sensitive on the subject of the epidemic, becomes seemingly indifferent after the invasion of the earliest stage, and is wholly regardless of life when he approaches the more alarming crisis. This apparent apathy has been ascribed, you are aware, to a disinclination to avow the apprehension, that the king of terrors has at length made his approach where most dreaded; but I cannot think that the true cause has been assigned. It is a symptom *belonging to the disease,* and I have frequently experienced a great difficulty in exciting the fears of intelligent patients so far as to induce them to submit to the necessary remedies and a proper abstinence from food. It is a condition of the mind, also, which is *progressive with the disease,* and I have rarely heard of the least solicitude being manifested, after the formidable stage has supervened.

The premonitory symptoms, here as elsewhere, were of comparatively short duration at the irruption of the epidemic, at which time death ensued in a few hours after the attack.

The treatment of this epidemic, in its incipient stage, must, of course, be determined by the particular symptoms of the case, the probable strength of the predisposition, general circumstances of the patient, &c. From the impaired condition of the digestive functions, it is indispensable in all cases to discontinue solid food, and to limit the quantity of farinaceous fluids.

1st. If the subject be of temperate habits, the tongue clean or slightly coated, the evacuations not particularly morbid nor frequent, and no vomiting, it will be generally sufficient to stop the solid food; or a small dose of castor oil, or of rhubarb and magnesia, will be all the medicine that will be indicated. If the diarrhœa be more urgent, denoting a more irritable state of the mucous membrane, probably two teaspoonsful of paregoric should be at once given, and another after each subsequent evacuation. A continued abstinence from solid food for a short time will supersede the necessity of a cathartic, where the bowels have been so thoroughly evacuated as a consequence of disease. The irritability of the mucous membrane is easily allayed at almost every period, and vomiting and purging arrested without much difficulty; and I have found paregoric the best remedy for that purpose when these symptoms are not violent.

2d. The next most simple combination of premonitory symptoms proceeds from some *error in food*, the most common of all exciting causes. If there exist much nausea, or imperfect vomiting, and the food have not evidently passed from the stomach, the patient will be immediately relieved by a mild emetic of ipecacuanha, associated with calomel enough to act as a cathartic. The organs of digestion are thus unloaded, and the predisposition to disease at once subdued. The patient should be afterwards restricted to broth or gruel, till the symptoms shall have been absent for a day or two.

If a patient, who has thus erred, be not seen till the contents of the stomach have been ejected, or have passed into the bowels, emetics are not indicated, for we have seen no benefit arising from them, except at the very invasion, and for the purpose of evacuating the stomach. At later periods they are nearly sure to kill. If the diarrhœa be now only moderate, and the evacuations not presenting the characteristic appearance of malignancy, and the vomiting have ceased, a dose of rhubarb and magnesia will probably suffice. But if vomiting continue with obstinacy, a stronger predisposition is shown, and calomel modified by a grain or less of opium and one or two grains of camphor, is indicated ; and if the discharges be not soon arrested, more opium should be given, and the calomel repeated once or twice if the secretions have appeared morbid, till the bowels are again moved. Or, when I think I have obtained a sufficient advantage from the specific action of that remedy, and it has not produced a cathartic effect, I suspend its exhibition and await that result for some hours ; or, if there be any motive for an early movement of the bowels, I exhibit a dose of castor oil. It frequently happens that the direct operation of a cathartic, at the very invasion of the disease, will alone arrest its progress ; but after a delay of a few hours, the mucous membrane may have acquired a degree of irritability that must essentially modify the practice.

The quantity of calomel which I exhibit at a dose in the premonitory stages varies from 10 to 25 grains,—generally about 20 grains. But I have observed a great difference in the action of calomel and other cathartics in the diseases of Boston and Montreal and those of this climate. Here we are more subject to congestive affections of the liver and the abdominal viscera, and more active doses are more advantageously employed than at the former places.

3d. If diarrhœa suddenly invade, and be soon followed by vomiting, without any obvious cause, and if the evacuations be serous, and resembling or approaching in appearance to rice water, the attack has arisen from the activity of the predisposing cause, and constitutes a formidable case. Here there has probably been a gradual accession of disease, but not denoted by any remarkable symptoms, till this sudden developement of diarrhœa and vomiting. In such a case the tongue may be clean, and scarcely any other morbid phenomena than the intestinal commotion and the serous evacuations to admonish us of the impending danger. If the pulse be not much depressed, this is a case for blood-letting, and perhaps for its repetition. At the same moment a grain of opium should be given, and repeated once in 20 to 60 minutes, till the irritability of the mucous membrane be subdued. A large mustard cataplasm should be also applied over the region of the stomach and liver, and as soon as the skin is irritated by that means, a large blister should be substituted. Among the early remedies, calomel should not be neglected, and conjoined with opium and camphor if the vomiting and purging continue; and it will be commonly found expedient to repeat the calomel for two or three large and successive doses. The evacuations will become very dark as the disease subsides. His convalescence will be gradual.

4th. If the patient exhibit a white tongue, and diarrhœa be almost the only other attendant symptom, he will probably be relieved by calomel alone. He will require particular prudence, however, in regard to food. If a sense of oppression at the chest exist, or pain in the head, or soreness at the region of the stomach, blood-letting will be indicated; and if tenderness of the stomach remain, a blister or leeches applied there will accelerate his cure.

5th. It sometimes happens that the earliest symptoms consist of headach, oppression of the præcordia, impaired appetite, and coated tongue. Blood-letting and cathartics of calomel are then required, and a blister over the region of the stomach. In these cases, where there has been no diarrhœa, the first evacuation will be very dark.

6th. If there be a yellow coating of the tongue, and purging of bilious fluid, with or without vomiting, the case is a favourable one, whatever the exciting cause, and requires cathartics of calomel and oil. I have, however, seen such cases result in asphyxia, and terminate in death, the patient purging and vomiting a deep yellow or a deep greenish-yellow bile to the last moment.

7th. Spasms sometimes introduce the disease, and may exist for some hours, unattended by any other prominent symptom. They have not, however, I believe, constituted a difficult case, but have soon yielded to blood-letting and cathartics. They sometimes also occur in connexion with the premonitory symptoms of diarrhœa, and require, with other means I have indicated, stimulating embrocations, friction, &c. The topical application of ice is also excellent for the relief of spasm, whether in the premonitory or the subsequent stages of the disease.

8th. There is occasionally, from the access of the disease, a very obstinate determination of blood to the head, and palpable inflammation of the brain. In such instances the evacuations in the primary stage are very light, or do not occur at all. These cases require decision with the lancet, leeches to the head, removal of the hair, and cathartics of calomel and castor oil. The dejections become, at once, very dark, and frequent cathartics will be necessary in the progress of the cure. These cases almost always yield to this treatment. When they do not, or are neglected, vomiting and purging of the characteristic rice water takes place, and the patient passes through the stage of collapse into a state of delirium.

Whenever vomiting occurs as a premonitory symptom, all fluids should be denied the patient. If it do not readily yield to the treatment suggested, ice held in the mouth will be found a useful auxiliary, and it affords the advantage, also, of allaying the intolerable thirst which is sometimes early attendant on that symptom.

If the symptoms denote a severe grade of disease, salivation will hasten convalescence, and obviate the necessity of some other remedies.

Although the predisposition to this epidemic is generally developed by some of the premonitory symptoms which I have indicated, it occasionally happens that the attack is very insidious. The tongue may be clean, and of a healthy appearance: the evacuations at first bilious, and unattended with pain; perhaps little or no vomiting and no evidence of danger in the countenance; yet I have known a sudden transition of these auspicious appearances to an array of the most malignant symptoms.

I have said but little in respect to the state of the tongue as an index of the disease, and still less of the pulse, and skin, and secretion of urine. They are so variously affected, under apparently the same circumstances, that they cannot be assumed as criteria. The tact of the physician, therefore, must convert them to the best advantage in each individual case.

The patient will require particular instructions in regard to food during his convalescence, as he will be very likely to yield to his propensities.

I regret that my time will not allow me to reply fully to your letter, but I will drop you another line this evening, and communicate, also, the results of some analyses of the blood.

In reply to your inquiry, I have had no symptoms of the Cholera, and the physicians here have been generally equally exempt. The few that have died perished in the good cause of humanity, and were the victims of a generous ambition, that prompted them to exposures which no constitution can resist.

Very respectfully, &c.

LETTER III*

New-York, *August* 19*th*, 1832.

Dear Sir,

In my letter to you yesterday, explaining the results of my experience in the treatment of the premonitory symptoms of Cholera Asphyxia, I had not the time to say what I could have desired on the no less important subject of the *exciting causes* of the disease. Having, however, adverted in a former communication to the general facts, relating to this inquiry, which have occurred under my observation, I shall now confine my remarks to the most common of all the exciting causes of Cholera. It is an argument, *à priori*, that the varieties of food and the moderate stimulants, which are well adapted to the healthy system in ordinary conditions of the atmosphere, can scarcely endanger the health of the being they were destined to nourish, under any vicissitudes of the natural phenomena of the elements. This very plausible induction has operated in most places that have been visited by Cholera, and the general sentiment has been in favour of a moderate indulgence in the fruits of the earth, and the luxuries of the culinary art. The irruption of the Epidemic, however, soon proved to us the fallacy of this philosophy, and the experience of the majority has been conclusive against many species of food, which they had hitherto eaten with advantage. The fact has been even appalling to the most incredulous epicure, who has been compelled to drop his accustomed varieties, as they have been returned, one after another, from his stomach undigested; or if perchance this organ have borne the indulgence with its wonted forbearance, an unusual tormina of his bowels has startled his confidence in the vigour of his digestive powers. Theory and experience all give way to the overwhelming argument of a choleric atmosphere. The constipated dyspeptic abandons his bolus of soap and aloes, and condemns unbolted flour as a purgative diet ; and even the shampooing of Halsted threatens him with hyper-catharsis. The doctors, who tickle the palates of their patients, are thrown into confusion, and some are alarmed lest it should lend a fact to another class, who regard the subject of food as an important part of the methodus medendi in diseases of the digestive organs. The epidemic influence, however, cannot be resisted, and there soon prevails a remarkable unanimity in regard to the impaired condition of the digestive functions, and the necessity of abstinence from many common articles of food. In short, there is an *internal impression* produced on every one, which admonishes him that he is more than usually vulnerable. The epidemic poison creates, almost universally, an irritable state of the stomach and bowels, of which very few are aware without an experiment, but, which is, however, commonly made ; and the issue will depend on the extent of the indulgence. Even the habitual drunkard glides safely along by abstaining from his accustomed liquor, and observing a rigorous diet ; and it has been constantly observed that in those subjects, the immediate exciting cause has been rather some indigestible food, than the inebriating poison. It is undoubtedly important to obviate constipation ; but this is rarely a difficulty in cholera times, and should be done by medicine rather than by food. The former, judiciously applied, is safe, the latter is attended with danger. The necessity of precaution in respect to ingesta will be more or less modified by the intensity of the predisposing causes. In this city it has extended, by common consent, to all garden vegetables and to all ripe fruits ; to pastry, puddings, cheese, and all the et ceteras which are not included in the following bill of fare. A healthy, robust man sits down to a breakfast of stale bread, butter, boiled eggs, and tea or coffee, or milk.—His dinner is constituted of some fresh animal food, light mealy potatoes, boiled rice, stale bread, eggs and a glass of water—perhaps of wine. For tea,—he contents himself with toast and tea alone. This is technically called a "cholera table," and prevails not only in private, but in many public houses. Not only, therefore, has it well been said that the Cholera is the "Apostle of Temperance," but it has taught mankind a lesson on sensual indulgences, which will not be soon forgotten.

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* This letter was introduced by the original publisher with the following remarks, and which are retained for obvious reasons. I need not say that I entirely concur in the opinion expressed on the subject of food.

" The author of this and two other able letters on the same subject appears to disapprove the use of fruits and vegetables during the Cholera season.

" On this opinion it may be remarked, that a distinction should be made between a place where Cholera is epidemic, and one where it is expected to be epidemic.

" Where the epidemic exists no one should eat fruit and vegetables, unless the state of the digestive organs require these articles, and then with all possible precaution as to quantity and observation of the effect produced.

" Where the Cholera does not prevail, the constitution of the atmosphere, although predisposing to the epidemic, need not prevent a prudent use of ripe fruits and good vegetables."

The great objection to fruit arises from the direct irritation, produced by its constituent acid, and from its particular liability to fermentation during the epidemic. I have known whortleberries to have simultaneously developed the cholera in five temperate and healthy members of one family, (the Bogart family,) who were all gathered to the tomb ere the setting of that sun which rose upon their sufferings.

Dr. Gale, a very intelligent chemist in this city, is engaged in a series of experiments on the blood of Choleric patients. He has supplied me with the following facts which may interest you: "In eight analyses of cholera blood," he says, "there has been a universal diminution of water, being, in some cases twenty per cent. and in one twenty below the ordinary standard, which is 9000. The salts, and especially the soluble ones, have been found from one fourth, to one half less than their usual proportions. The colouring matter has been found to have increased in some instances to eight or ten per cent. This blood, when drawn from the patient, appeared like the dregs taken from a cask of old port, of a muddy purple."

A fact worthy of remark is, that those who have long suffered from the premonitory symptoms, have lost both the water and salts of the blood in proportion to the profuseness of the evacuation; which renders it probable that the loss of water and salts from the fluids of the system would occur from any profuse evacuation.

The principles of the blood which have been lost, have been found both in the evacuations and the perspirable matter.

Very respectfully, &c.

P. S. It has been contended, that a critical reference to food produces a morbid sensitiveness on that subject, and that this fearful apprehension is likely to impair still more the healthy tone of the stomach. The argument, however, is not sustained by the facts. It is fear which impels us to an observance of food and to avoid the exciting causes. When public attention is properly directed to this subject, and the exciting causes are well understood, the fears, which agitated the individual, are allayed, and he eats his food with a conviction of its inoffensiveness. This is conclusively shown by the almost universal exemption from cholera of such as are properly abstemious; and I have had the most ample demonstration that the victims to this disease, among the sober members of society, have been such as have disregarded the admonitions of experience in relation to food.—An attempt at concealment is natural enough, where the sufferer has been in some measure the victim of his own imprudence; but I think that society requires the benefit of every illustration of this subject, and I do not hesitate to declare my conviction, that in most instances the disease, in this city, has been the direct result of some error on the part of the individual.

LETTER IV.

New-York, *August* 22, 1832.

Dear Sir,

A few days ago, in replying to your letter, I did myself the honour to state to you the results of my own observation, and that of others in whom I have particular confidence, in the treatment of the "premonitory symptoms" of Cholera Asphyxia. These symptoms continue to announce the approach of the more malignant stage, with rare exceptions, and are still susceptible of easy removal. Positive injunctions must be given to the patient, to deter him from injurious indulgences during his convalescence, a propensity to which, I have had occasion to remark, is a characteristic symptom, and which frequently involves the sufferer in a most abrupt transition to the fatal stage of collapse. To effect an observance of the proper objects of attention, it is not unfrequently necessary to excite the fears of the individual.

I adverted in a former letter so very briefly to the treatment of the cholera in its advanced stages, and as the disease has not yet presented itself to your observation, I have determined to avail myself of a few moments of leisure, to submit to you the experience of the best physicians in this city. There was, at the irruption of the epidemic, a greater diversity of practice than prevails at the present time. The fatality of the disease, and the different views which were entertained of its proximate cause, necessarily suggested many remedial agents of conflicting principles, and every subordinate expedient that ingenuity could devise. I always commended this enterprise and resource of the profession, although it was sometimes opposed to my own prepossessions. The experience of foreign practitioners was only conclusive as it related to the epidemic in their own climates. It had here invaded almost another world, and it was fair to presume, that its character would be modified by the great physical differences of climate, and a thousand local accidents. It was a just presumption, that what was true of one remedy in Asia, and of another in Europe, might be found inapplicable to the modified disease in America. There was, and is, nothing settled in regard to the proximate cause, and, therefore, we could derive but little assistance from established principles. Our only guide was existing experience, and our only hope, in the favourable direction that might be given to experiment by modifying circumstances. Each practitioner, therefore, met the destroyer with such weapons as experience had provided, or his own ingenuity suggested. Their incongruity was certainly indicative of the undetermined nature of the disease and of its great fatality Each system of practice, and almost every remedy employed, have been fully and fairly applied; and after a careful observation of their results, I am prepared to say that we have gained nothing since the disease invaded Europe. It is the same disease here as it was in Asia,—but little modified by those great physical causes which distinguish the different climates of the earth, and which not only generate peculiar diseases, but exert the most sensible influence on the moral and physical condition of man. If less distinguished as a specific disease than those of the great contagious family, it is certainly not less remarkable in the uniformity of its phenomena; and it is almost equally characterized by pathognomonic symptoms. This identity, also, is further illustrated by the coincidence in the results of our treatment, and that which has been applied in other parts of the globe. From this rule, however, must be excepted the very important remedy of blood-letting, which was found so beneficial in the Indies; but even to this we have not given that full opportunity, on which the Anglo-Asiatic physicians insisted—having either been baffled in our attempt at its abstraction, or we have not carried its effect beyond that deliquium which results from the removal of some dozen or fourteen ounces, and beyond which, we are told, we shall reach the great centre of the circulation, and re-action commences. Although the moderate depletion by this remedy, which has been often practised by judicious hands in this city, has seldom encouraged any hope of success, I am determined to employ it, in extenso, when favourable opportunities may occur. I will confine it to those cases in which any experiment may be justified, and where the diarrhœa has not been profuse. The arteries will supply us with any quantity,—and I know not that 25 or 30 ounces of blood, so obtained, can more endanger the patient, than an exudation of 200 or 400 ounces of serum from the mucous membrane of the intestines. The disease, however, is now happily passing from this city, and should you be so ill-fated as to suffer a visitation, I shall look to you for a decision of this most important inquiry.

I entertain no doubt, that the greater success of this remedy in the hands of the Anglo-Asiatics is to be ascribed to the absence of premonitory diarrhœa in the East, and the retention of the fluids in the system when blood-letting was so usefully practised. It is equally apparent, also, that we may depend on a greater probability of success, should we be so fortunate as to meet with the blood not already deprived of its serum.

In regard to *calomel*, my own experience is sustained by that of our best physicians. From the particular irritability of the mucous membrane, and the profuseness of the discharges, the prepossessions of many physicians, who now regard it with a sense of dependence, were adverse to its exhibition. The little encouragement, however, which they derived from other remedial agents, directed their attention to the declaration of foreign physicians, that this medicine not only allays the irritability of the mucous membrane, but induces that change of action which is necessary to the re-establishment of health. I may safely assure you, Sir, that our best practitioners now employ calomel in doses of 10 to 30 grains, with more confidence than they repose in all other remedies. Either with or without small quantities of opium and camphor, according to the existing state of the serous evacuations, it seldom fails to allay the irritability of the mucous membrane, and of the system generally; to mitigate or remove the spasms; and if the case be susceptible of relief, to convert the discharges from the appearance of rice water to that of the green confervæ, or the not infrequent blackness and consistence of boiling tar. This transition is nearly a sure omen of a radical change through the whole system. Then are we delighted with an almost simultaneous developement of the great functions of life. The action of the heart is no longer almost extinct; we press the arteries with a thrilling sense that Death himself is subdued; we are warmed into hope and joy by the returning heat to the extremities; the hand no longer burns on the region of the epigastrium, where the heat may have borne a contrast at 106 degrees; the features begin to expand, and the prophetic blueness to fade away into the flush of life or the vermilion of a less dangerous congestion; and although a spectacle, rare in opposition to others that have baffled the same skill and the same physical means, we contemplate the result even with admiration, so great is the victory, and so obvious the triumph of art.

Although calomel thus transcends all other resources in the treatment of Asphyxiated Cholera, there are subordinate agents of indispensable importance. These consist chiefly of all those means, which, applied externally, stimulate the skin and excite the vascular action of that organ. Heat is the most important, and is commonly applied by accumulating around the patient bottles of boiling water, bags of hot sand, the air bath, &c. Hot chalk is an excellent application when the skin is wet with perspiration. Stimulants of every kind have been employed, and none more universally than mustard cataplasms to the extremities and abdomen. It is my practice to apply a very large blister over the abdominal region, as soon as there is the least probability of its making any impression. An ointment has been introduced at the Greenwich Hospital by its intelligent and ingenious physician, Dr. Roe, and adopted with evident benefit at other hospitals and in private practice. It consists of one pound of strong mercurial ointment, seven ounces of Cayenne pepper and seven ounces of camphor, intimately blended. The whole surface of the patient is thoroughly rubbed with this composition, by means of a brush, and the process frequently repeated till reaction ensues, or salivation is effected. Another ointment has been compounded by Dr. Rhinelander, of the Crosby-street Hospital, and is prepared in the following manner: Take of red pepper and camphor each half a pound, simple ointment one pound, muriatic acid one ounce. Triturate together the powdered camphor and pepper, adding the acid. Pour the mixture into the melted ointment, carefully stirring it. The ointment is very freely applied to the whole surface, and the skin afterwards covered with hot chalk. In this preparation, the active principle of the pepper is evolved by the acid, and it possesses, therefore, the advantage of being more stimulating than the other, but it wants the specific property which is imparted by the mercury. Dr. Roe's prescription is found an excellent adjuvant, in co-operation with calomel, in producing the constitutional effect of that substance, and, with proper subsequent treatment, a patient is rarely lost after salivation is established. Various other stimulants, more or less compounded, are in use, and are all found preferable to dry friction.

The *hot bath* has not been much employed, and has been considered inferior to the other means of applying heat. I once assisted some medical friends, in immersing a patient, whose case presented all the striking phenomena that are described by the oriental physicians, in a bath in which was diffused a pound of strong flour of mustard. I have seen no case since, terminating fatally in so short a time, in which reaction was more fully effected, and I have not since repeated the experiment. Time for action is short, and there has never been a tub or water at hand. From the result of that application, although the patient died soon after, I think the mustard bath is worthy of further trial. Dry heat by means of burning alcohol has been fully applied, but with no very useful effect.

I have been long in the habit of employing *ice* for the relief of vomiting, and it is found essentially useful for the same purpose in Cholera Asphyxia. It also allays the thirst, and thus removes one great cause of restlessness. How far it exerts an impression on the morbid action is a subject of inquiry. An extraordinary degree of heat is evolved about the region of the stomach. In one case, in which I applied the thermometer to the surface, a temperature of about 106 degrees was denoted while the patient was expiring. I have imagined that these cases will be more generally benefited by the external application of ice to the epigastric region, than by mustard cataplasms. Ice, too, I have applied advantageously to the head in obvious congestions of the brain, which are frequently attendant on Cholera, or arise as one of its consecutive diseases. It is also very advantageously applied, for the relief of spasm, to the muscles affected.

Stimulants, internally, have been almost unanimously employed, and by some to a very great extent. We have seen but little benefit arising from them, and least of all from opium given as a stimulant. I think the Cholera has conclusively shown that its mode of action is *sui generis*, and not analogous to that of alcohol. In very moderate doses, it is useful in allaying the irritable state of the mucous membrane, particularly in restraining the evacuations; but, beyond this purpose, I believe it never fails to do harm. The best of the profession, and who have seen most of the disease employ alcoholic stimulants in great moderation, and generally prefer the more volatile and diffusible. Their use is clearly suggested by the failing powers of life; by the very languid state of the circulation, and other phenomena incident to approaching dissolution. But, in opposition to these symptoms, they are contradicted by the pathology of the disease,—at least by some of its local developements. We have seen no benefit from their liberal use, and it is even doubtful whether they contribute much in any quantities. It requires the conviction of experience, however, to enable us to abstain from their use, and to resist the impulse to apply them to the dying spark.

One of the best properties of these agents is that of allaying nausea and vomiting, particularly diluted brandy. Dr. Arnoldi, of Montreal, informs me that he employed with great success small quantities of brandy and strong coffee in combination; but I have never tried it.

Emetics have not been the subject of much experiment. Experience in the old world has admonished us of their danger; pathology is opposed to their exhibition; and the alarming irritability of the mucous membrane, and the terrific prostration of the vital powers, have deterred us from their use. We have hastened to quiet the tumult by more sedative or by exciting means of another character. Where they have been employed in Asphyxiated Cholera, I think

I am warranted in saying they have hastened the fatal termination. Where they have been employed with success, I have no doubt it has been in what is regarded as the forming stage of the disease.

In the progress of convalescence, emetics are occasionally salutary, but it is only after an absolute impression has been made on the morbid action. It has been very judiciously observed by a committee deputed by the Kappa Lambda Association, in their publication on this disease, that, "after the system begins to react, and the calomel to operate, there is a discharge of immense quantities of dark porraceous bile, like boiled spinage, and this often collects about the præcordia, creating great distress, even so as to prevent the patient from lying on either side. This very troublesome symptom has been found to yield immediately to the operation of an emetic of warm salt water, or of Seidlitz powders repeated at intervals." The result of the observation of the committee is of course correct, although I think the philosophy wrong.

Cathartics. I have not placed calomel in connexion with this class of remedies, as its superior advantages in Cholera depend on its specific mode of action. Its ultimate operation as a cathartic is doubtless desirable, but not essential to the changes it exerts. When we have accomplished all that we contemplated in the preliminary treatment of Asphyxia by this remedy, and a cathartic is indicated, none answer so good a purpose as castor oil. I have long been satisfied, though I do not recollect to have heard or to have seen the remark, that castor oil exerts a specific action on the liver, and sustains a rank, in that relation, only subordinate to the mercurial preparations. It manifestly co-operates with the latter in the production of bilious discharges, and will, independently, produce that result more than any other cathartic. It possesses, also, the eminent advantage of generally irritating less the mucous membrane, or prostrating the strength. Dr. Caldwell, of Montreal, informs me in a letter to Professor M'Naughton and myself, that he employed as a cathartic, after the exhibition of calomel in the earlier stages of this disease, tart. potassæ ʒij. in a little warm chicken broth, which he repeated every second hour until it operated freely. "Nothing," says that eminent gentleman, "seemed to allay the gastric irritation so well as the soluble tartar. In many instances it seemed to act as a charm, and I do not know a single instance of any one, who went through the influence of this treatment, that was subsequently attacked with Cholera." In the progress of the case, rhubarb and magnesia, especially when combined with more than an equal quantity of Rochelle salts, answers a good purpose. The salts, probably, by accelerating the operation, prevents the rhubarb from irritating the mucous membrane. The common saline cathartics, uncombined, irritate the bowels, and are generally prejudicial. I know but little of the action of the resinous, not having employed them from their irritating properties. I have seen Asphyxia produced by their severe operation. Annesley often combined aloes with his scruple doses of calomel to effect more perfectly the evacuation of the pultaceous creamy matter, which often filled the intestines, and to the presence of which he ascribed so much evil. We do not find this an attendant symptom, to any great extent, of our choleric patients. I think also that an objection applies to the irritating properties of senna, and I believe it is very rarely used. Charcoal is given by a few as an adjuvant to other cathartics. This class of remedies is clearly only indicated after a considerable change in the morbid action has been effected by other agents.

Camphor has had in New York its few warm and respectable advocates; but the great proportion of the profession have not the least confidence in its remedial properties. Its cause has not been sustained by any authenticated cases, and when employed alone, it has been perfectly abortive in my own hands. It is directed to be exhibited in doses of three to five drops of the saturated tincture, frequently repeated; and to be employed, at the same time and in larger quantities, in the form of enemata. It is very useful in allaying some of the symptoms of the "precursory stage," and is an excellent adjuvant in subduing the irritability of the stomach and bowels, and controlling the spasms in the advanced stage of Asphyxia. For this latter purpose, also, it exerts a good effect when applied externally. It imparts to opium a more sedative effect, and forms with that substance and calomel an admirable combination when vomiting and diarrhœa exist. It is liable in large doses to produce determination to the brain. When employed as a specific, its exhibition proceeds on the homopathic plan.

Opium and its preparations. I have briefly adverted to this substance as employed by some physicians with a reference to its supposed stimulating properties, and have explained the general result in the use of it. When given in excess, it seldom fails to aggravate all the phenomena of congestion, and has done more mischief in the premonitory stages. It is valuable when cautiously employed, and it not unfrequently happens that one preparation is preferable to another. In a former letter, I indicated some of the conditions of the premonitory stage in which it was evident that the camphorated tincture was best. Dr. Tappan, of the Park Hospital, informs me that he exhibits with the first dose of calomel about ten grains of Dover's powder with a happy effect; and I may here remark that it is frequently his practice, after the plan of some Anglo-Asiatics, to administer a full dose of opium immediately after bloodletting in the less advanced stages of the disease; a practice which is sustained by the report of the Kappa Lambda Association. I have found in the *sulphate of morphine,* a preparation, least of all liable to objection. Its astringency is not sufficiently great to arrest any profuse diarrhœa; but where it is chiefly an object to allay the irritability of the stomach, to subdue the spasms, and to tranquillize the system, its effect is admirable, and its tendency to increase congestion comparatively little. When purging and vomiting exist in connexion to any great extent, it is useful to give the sulphate of morphine, and to administer the tincture of opium by injections. The *sulphate of quinine* has been employed in a few cases at the Crosby-street Hospital, with and without calomel, in doses of ten grains, once in an hour or two. The effect has been uniformly rather bad, and the patients have died. *Charcoal* has been employed by a few physicians, and is in still greater repute as a popular remedy. By the latter class, it is of course only exhibited in the mild stages of the disease ; and when any apparent benefit has arisen from it, the subjects, I believe, in all instances, have only been affected with the premonitory symptoms. It certainly can exert no effect on the vital properties, and, of course, does not reach the functions through the only media by which such a disease can be subverted. Its action is evidently merely chemical, as an absorbent, and laxative by its mechanical irritation.

Injections are very subordinate remedies, and have been most advantageous when employed to impart heat to the intestines through the medium of water, or restrain evacuations by the agency of opium. Large quantities of hot water have been sometimes absorbed in an hour or two, after being injected. At other times it has been removed, at a very reduced temperature, without any sensible diminution of its quantity. This is probably a better mode of administering alcoholic stimulants, than by the mouth. The weight of experience here is more in favour of this method. When vomiting does not exist in connexion with diarrhœa, it is also better to restrain the latter by injections of laudanum, than to introduce much opium into the stomach. Composed of warm water, they are useful auxiliaries in the progress of convalescence, to relieve the tormina which sometimes proceeds from the irritation of the acrid bile. An infusion of *tobacco,* according to the suggestions of Mr. Baird, was kindly injected at my request, in one or two cases at the Crosby-street Hospital, but evidently with a bad effect. Other gentlemen have informed me that they have derived no benefit from its use. A solution of the *super sulphate of alumen* was freely administered per anum et per orem, at the Bellevue Hospital, where it was used on the principle of its astringency; but I believe the very enterprising gentlemen of that institution were disappointed in its effect.

Cupping and *leeching* have had a full proportion of our attention. They are less salutary than general blood-letting, when it is an object to produce an impression on the whole system, or for the relief of high vascular action, whether local or general. In Asphyxiated Cholera, where there exists more than the usual determination of blood to the head, they have been found useful, and contribute to the relief of oppression at the precordia, and some of the varieties of abdominal distress. Like general blood-letting, the local abstraction is often indispensable, in the treatment of some of the consecutive diseases.

Transfusion, according to Dr. Latta's method, has been tried, and found wanting. It has been employed in numerous instances at the hospitals, and in private practice. I have injected but one patient, who was surprisingly benefited for a short period, but who soon relapsed into his dying condition, and expired in three hours after the operation. The results of this experiment have been generally uniform. The circulation is re-established, and sometimes with considerable vigour ; the temperature of the body is universally augmented ; the cold perspiration is converted to an animating warmth ; the oppressed respiration is removed or relieved ; the livid hue of the features is changed to the flush of health ; the general restlessness is subdued ; and the patient exclaims that his cure is almost completed. I witnessed an interesting exemplification of these phenomena, in a very robust subject, at the Crosby-street Hospital, who was suffering the whole array of symptoms that distinguish the collapsed Cholera. The operation was performed by Dr. Depeyre, and the solution injected stood at the temperature of 114 degrees. A thermometer was introduced within his mouth, and the temperature marked at 93 degrees. Three pounds were slowly transfused, and the thermometer had risen to 101½ degrees. All the evident improvement I have noticed above immediately ensued, and the man laughed aloud from the ecstacy of relief. This man was a corpse an hour after. These results embarrass the hypothesis of the humoral pathologists, and supply another argument to those who assign the proximate cause to a modification of the properties and functions of organic life, and who regard the alteration of the blood and the other fluids as consecutive, and only one of the numerous symptoms which arise from morbid action.

I can only learn that "Dr. Leo's Method" has been tried at the Greenwich Hospital, notwithstanding its claim of novelty, and the well-known properties of his remedy. It was found wholly inert.

Electricity has received but an unsatisfactory trial, though I am informed that it has produced no sensible effect.

The hypothetical treatment of Dr. Binaghi has been put to the test, especially at the Crosby-street Hospital, and has added another demonstration of the inefficiency of remedies that are directed to the primary impression of a morbific poison, acting, by concession, through the *medium of the nervous system*, but having involved every other tissue, and deranged the whole economy of organic life. The moxa, burning alcohol, et id omne genus, having had their day, no longer torture our patients in vain. The sulphate of quinine, I have said, has been also insufficient in the few instances in which it has been given ; but as to strychnine, brucine, &c. commended by this writer and Haaz, I cannot learn that they have received any notice; nor do I know that hyosciamus and cicuta have been applied as remedial agents.

The *oxygene* and *nitrous oxide gases* have been tried at the Greenwich and other hospitals; but I am informed by the gentlemen connected with three of those institutions, that they have obtained no benefit from their use,—and that the blueness of the skin was not even influenced.

A negative part of the treatment, of very great importance, consists in the exclusion of all food, and even of water,—so necessary is it to subdue the irritability of the stomach, and so easily is that organ offended.

I had intended, when I began this letter, to have illustrated our practice by its application to particular modifications of the disease. There are very few who have any confidence in the specific action of any remedy; and that treatment can only be successful which is determined by general principles, and the peculiarities of each individual case. The best indications are frequently derived from idiosyncrasies, and where these are neglected the greatest advantages are lost. I have no doubt that it is owing much to this circumstance, that blood-letting has so constantly failed at our hospitals, where the subjects are broken down by intemperance, or by previous disease. Too much praise, however, cannot be given to those gentlemen who have employed a remedy so philosophical, and yet so opposed to prejudice, when it was obvious that empyrical practice, or even the power of calomel, must fail. Since my preceding observations on the subject of blood-letting, I have been informed by Dr. Dekay, that, in his private practice he has adopted this remedy, with the best success in very advanced stages of the disease, when the constitution of the subject was adequate to a co-operation with art. This gentleman exhibits diffusible stimulants freely at the same time, with large doses of opium, which he regards as essential to the success of blood-letting. He has not observed any deleterious effect from the opium, and its tendency to produce congestion is counteracted by the impression from the abstraction of blood.

Very respectfully, &c.

LETTER V.

New-York, *August 25th*, 1832.

Dear Sir,

Having hitherto restricted my letters to practical remarks on the Cholera of New-York, I shall venture to digress from experience to theory; although I do not imagine that you can be much interested by any communication, which does not relate to the important facts that appertain to this subject. Still, the propensity to speculate on these facts becomes irresistible; and however one may, at first, resolve not to entangle himself in the mazes of hypothesis, he unavoidably arrives at principles which influence his judgment, and which finally become so important to himself, that he cannot believe that they will not be interesting to others.

The first opinion I intend to examine is that which places the proximate cause of the Spasmodic Cholera in the bowels, and which supposes *inflammation* of those organs to be the source of the whole mischief. This doctrine is insidious, and derives great plausibility from the frequency of the small intestines exhibiting a pinkish appearance,—from the occasional softening of their tunics,—from the vomiting and purging,—and from its being capable of explaining many of the morbid changes that arise in other parts of the system, on the principle of sympathy. But inflammation, to produce such extensive sympathies, and such a rapid succession of phenomena, so soon resulting in death, must be severe. Its existence, however, is scarcely denoted by many of its usual characteristics. There is never any hardness of the pulse, no cupping of the blood, and scarcely a pelicle of coagulable lymph,—abstracted with whatever care,—no tenderness of the abdomen and no continued pain, no remarkable prostration of the muscular strength, no dryness and no redness of the tongue or fauces, and no symptomatic fever. Pathological anatomy is scarcely less conclusive against this theory. Our own necroscopic researches correspond with the observations that have been made in the other hemisphere. We look in vain for that florid redness which distinguishes acute inflammation. Its hue is dull, nor does it exist in those minute vessels which are the instruments of that mode of disease. It is evident, that this appearance exists in the venous and not in the arterial system. The larger arteries, leading to the intestines, are exhausted of blood; the veins are commonly full, and there is never presented any of that minute injection, which would be necessary to the existing condition of the larger vessels, did the redness reside in the terminating series of the arterial vessels. But this redness does not uniformly belong to the mucous, and may then be found to exist in the serous tissue ; as was observed in the case of the black female which I reported on the 2d of July, and which is noticed by others. The constitution of these tissues is so different, and the ordinary results of their morbid affections are so characteristic of their peculiarities, that striking modifications should at least arise, when inflammation of one or the other might happen to be the proximate cause of Asphyxiated Cholera. The almost constant pathognomonic symptoms of this disease, the very uniform combination of more accidental phenomena, and the analogies supplied by other tissues of the system, are certainly opposed to an hypothesis which ascribes the seat of the inflammation, indiscriminately, to the mucous or the serous organs. And here let me remark an important circumstance, which adds to the obscurity of the subject. Notwithstanding the redness be wholly confined to the serous tissue, the discharges of serum from the mucous membrane are as abundant, as when that organ is the seat of the vascular fulness. These are phenomena which require investigation. This irregular developement of congestion, and sometimes of inflammation, in one or the other of these coats of the intestines, will doubtless account for the tenderness of the abdomen in some instances, and for its absence in most. It has, however, happened in many instances, that the appearance of redness is wholly wanting, and we are then told that the nature of inflammation is not understood, and that redness is not necessary to its existence. Thus driven from the most difficult entrenchment, the hyperæmia of Andral is preferred by some, for the still greater ambiguity of the term, and its more generic import. But, yet another sign. The mucous tissue is softened,—it is disorganized,—and therefore is denoted the most active inflammation. When was it proved, I would inquire, that no other mode of action is capable of producing such a change? Who ever, before, witnessed the Cholera Asphyxia? and may there not be something in that disease, differing even from inflammation, that may modify or derange the structure of organs? What action is that which precedes mortification of the lower extremities in subjects of an advanced age, without any apparent exciting cause, or any sensible evidence of inflammation? The morbid action of Cholera sometimes tends to the disorganization of the whole body. We see it in the blood before life is extinct,—we recognise the withering poison in the frequent flabbiness of the heart,—and we are sure, from the rapid decomposition of the body, in some cases, that there had been almost a conflict between the vital properties and the laws of chemistry.—I will not illustrate these phenomena by the operation of the electric fluid,—for I believe that they arise not alone from the absolute extinction of life, but owe much of their production to the elements of disease. This argument, however, like the last, is deficient in the uniformity of facts. The softening of the mucous tissue occurs without any other sensible mark of inflammation, or does not exist where inflammation may be obvious ; and in a majority of cases it does not occur. It is sometimes seen, and again is absent under the most similar circumstances. The most remarkable instance I have witnessed occurred in the black female to whose case I have alluded, and whose disease was of short duration. I accidentally omitted the fact in my report of that case, but every tissue of the small intestines was so softened, that they were easily penetrated by my finger. It will be recollected that the appearance of congestion, in that case, was confined to the serous membrane. It is said that the glands of Brunner and Peyer are enlarged, and that thus this particular species of morbid action is denoted ; but this argument, like the others, must fall from want of the fact. I have known it to have been absent in at least an equal number of the many cases which I have seen examined, and the weight of the best authority is against it. I have seen these enlargements in some instances, the evident consequence of an antecedent chronic diarrhœa.

Perhaps the fallacy of this doctrine, which I have endeavoured to invalidate, may be illustrated by the recital of a single case, though others analogous are constantly occurring. But a few days since I was summoned in the morning to attend a young lady who had been attacked in the night with malignant Cholera. She had been for a few years rather delicate, and susceptible of congestive diseases of the abdominal viscera. Since the irruption of the epidemic, she had been twice affected by diarrhœa, but which spontaneously subsided. The last attack was four or five days anterior to the catastrophe of which I am speaking; and during the intervening period all was quiescent. I entered her chamber at fifteen minutes before seven o'clock, and her hyppocratic countenance expressed but too truly, that the destroyer had almost done his work. Her case, therefore, may be briefly told. She had felt entirely well on the preceding day, and up to the hour of retiring at twelve o'clock. She had eaten for her dinner of hashed-beef, a few stringed beans, a light mealy potato and a small beet. The repast was finished with baked rice pudding and a sweet sauce. I believe, however, they were all taken in moderation. At her tea she took nothing more than toast. Between eleven and twelve, prompted by a good appetite, she ate again of the cold pudding and sauce, and retired to obtain the sleep from which she was destined to awake in death. At four o'clock the invasion took place, and from the progress of dissolution, when I saw her, she must have begun to die at that hour. At half past 10 o'clock, life was perfectly extinct. Here is a violent, and a rapid case, and did the symptoms denote that aggravated inflammation of the bowels, which should have existed, had such been the source of the phenomena? Nothing can be inferred from the pulse, for that was almost insensible.—The tongue was as natural as in health.—The more important evidence of tenderness of the abdomen was totally wanting,—and the profuse discharges of serum were neither preceded, nor attended with pain. The patient protested against a cataplasm to the abdomen, "as she had been much more severely affected before, and had recovered without any remedy."

I am far from believing that the pathologiac state of the stomach and intestines has not a very important connexion with the disease, and exerts a most positive influence on the whole system; augmenting the violence of the constitutional symptoms, and precipitating death. The common developement of the earliest symptoms of disease occurring in those organs,—the profuse and morbid secretions which soon ensue,—and the very general redness of the mucous or serous tissue,—render it evident, that there is a particular determination of the morbid action to those organs;—and their well known properties, and important relations, can leave as little doubt, that the whole circle of sympathies is variously and extensively disturbed. The astonishing discharge of serum from the mucous membrane cannot be the result of a mechanical process. The vital properties, and the modified functions of the extreme vessels, must be concerned in the production of this fluid; but the secretion is so rapidly performed, that the component parts of the blood are separated, without supplying that usual evidence of change, which marks the productions of analogous processes. With as much propriety may it be said, that the profuse discharge from the skin is a simple exudation from the blood, as the separation of the serum by the mucous membrane may be attributed to the same mechanical principle. Whether the mode of action be always the same, or what particular modifications may arise, it will probably be difficult to determine. Dissections must be made with an especial reference to this inquiry, which may yet reflect other light on the pathology of inflammation. It may yet appear that the fluidity of the blood is so affected, that its red globules can no longer enter the extreme vessels; or perhaps, that the depletion of the instruments of disease has so modified their vital properties, that irritability is rendered more obtuse, and the red globules recede to the capillary system. Certain it is, that the blanched appearance of the large intestines is generally greatest as the disease has been protracted, and the discharges profuse. I know not whether it be true, that there exist more of the common evidences of inflammation in subjects that have not been affected with vomiting and purging, as such instances are rare, and I have not witnessed, nor can I learn that dissections have been made where death has occurred under these circumstances. I very recently attended the examination of a robust subject at the Crosby-street Hospital, who died in a few hours after transfusion with the common saline solution: His habits were intemperate, and the disease was severe and rapid. The examination was made about eight hours after death. In this case, the loss of serum had been partially supplied, and of course, the blood was more than usually dilute. The small intestines appeared to several observers, who had witnessed numerous dissections at this and other hospitals, to exhibit a more vivid redness than was recollected in any other instance. The mucous tissue was very minutely injected, and presented, through its whole tract, a uniform blush of redness. It was studded with enlarged follicles, but its texture was firm as in health. There was nothing more observed that can illustrate my subject; but it may be stated, that the large intestines were rather blanched, and its mucous follicles were not visible; that the stomach was large and free from every vestige of disease; that the liver and other viscera of the abdomen were natural;—the gall bladder full of greenish yellow bile, none of which had escaped into the intestines;—the bladder perfectly contracted;—the lungs partially inflated and entirely natural, and the heart soft and flabby;—its cavities and the larger vessels were well filled with blood, nearly as dark, but rather more fluid, than in those who die without the saline transfusion.

It is not improbable, also, that a contraction of the extreme vessels, may occur extensively through the system, by which the red globules would be further excluded.

Although I do not advance these conjectures as probable, and believe that the absence of the usual phenomena of acute inflammation is strongly indicative of some other mode of action, I could still desire, that further investigation may enable us to resolve this morbid state of the intestinal canal into a modified form of inflammation. The treatment, not only as respects these organs, but also the influence of their diseased action on the whole system, would then be more within the reach of philosophy, and those principles which experience has established. But whether inflammation, or some mode of action essentially different, I cannot doubt that it is only a part of an original series of morbid actions as extensive as the numerous functions of the system.

There is one analogical fact, in the view of the subject just taken, that may encourage the presumption that the action is allied to the proximate cause of inflammation. It is now generally conceded that profuse hæmorrhage from the mucous coat, whether of the stomach or intestines, is frequently the direct result of acute inflammation, and proceeding from the secretory organs of that membrane. It appears, therefore, to be a fair suggestion, whether the fluid evacuated, in a great proportion of choleric subjects, may not be the product of an action analogous to inflammation. We have all the constituent parts of the blood with the exception of its red globules; and it may be farther remarked, that as in patients affected with

Cholera, the discharge is generally sudden and profuse,—so also in hæmorrhagic inflammation is the evacuation of blood surprisingly abundant;—and under either circumstance, the system sustains the depletion, with less prostration of its powers, than would arise from any other mode of abstracting the vital fluid. Perhaps not the least remarkable incident attending this secretion of the component parts of the blood, is the great uniformity of its occurrence wherever the Cholera has prevailed. But opposed to these conjectures, which may be advanced by the advocates of inflammation, is nearly the whole symptomatology of the disease; and let it not be imagined, that in having thus given a fair representation to the subject, I am already becoming a convert to the doctrine which regards inflammation of the bowels as the proximate cause of Cholera. I am not only willing, but could even desire, for the reasons which I have assigned, that it may be yet determined that this mode of action is analogous to something with which we are familiar, and the treatment of which may be embraced under the settled principles of our science. We shall have accomplished much, if we can relieve the intestines of the weight of disease which they so generally sustain, and in doing which the constitutional derangement will be released from an overwhelming sympathy,—and the blood vessels will be no longer exhausted.

Have the *Humoral Pathologists* supplied us with a better solution of the great problem of this disease? I cannot believe that their philosophy will bear the analysis which must ultimately sustain the conclusion at which reason aspires. The most critical examinations do not detect any foreign substance in the blood, nor have its elementary or component principles formed any new combinations. If the due proportion of its constituent parts have been disturbed, and we find a diminution of the serum and the salts, by carrying the same investigation a little further, we shall discover them in the form of secretions, and by that very circumstance, denoting the essential agency of the solids in their separation. But before this diminution takes place, and while yet there are ample phenomena to evince the malignancy of morbid action, we find the blood still constituted as in health, and only yielding up its physical characters to the organized parts of the system. It is only as disease advances, that this vital fluid succumbs to the influence of the modified powers and functions; and at last when dissolution begins, the changes have become quite apparent. If diarrhœa or vomiting have attended the case, we discover an absence of the serum or the salts in the ratio of those discharges. Hence we are told, that the problem is demonstrated, and that we have nothing to do but to *replace the evacuations.* It is then that the lungs refuse to perform their office, and respiration is almost at a stand. What is the unavoidable result of the interruption of this function, no less remarkable than the impaired action of another organ, on which so much emphasis is laid? It is an imperfect oxygenation of the blood—and therefore "*oxygen gas* is a specific for the disease." This failure of decarbonization can never be detected till evident lesions have occurred in some of the great organs of life. Does the blood sometimes refuse to separate into its component parts? This is only so in the advanced stages of the disease, when other sensible changes have been the slow result of the morbid process, and may be regarded as a rare phenomenon if the blood be properly abstracted.

Perhaps the most remarkable appearance attending this fluid, and which I announced in two cases lately reported to you, as the discovery of Dr. Gale of this city, is the extrication of the oil of the blood, and its existence in a free state in every part of the vascular system. If any other accidental deviations in the sensible properties of the blood have been sometimes observed, as its diminished fluidity where vomiting and purging had not existed, it has only been when the organic powers have suffered in more than a corresponding ratio; and the complexity of that fluid, and its probable vitality, will afford modes of accounting for such deviations, and render the principle of explanation which I have adopted sufficiently comprehensive. Do we find in the fluids secreted any evidence of a morbid condition of the blood? Let us examine the results, as the extinction of life approaches.—We detect nothing more in the evacuations from the alimentary canal than the component parts of the blood itself. The organs of secretion have scarcely produced any change in their properties, or their relations to each other, and it is the fairest inference that there is nothing here to countenance the opinion of vitiated fluids. If Christie, Annesley and others, discovered in the intestines an opaque, pultaceous fluid, its existence did not occur to other observers, and I believe has been rarely noticed in this hemisphere. The acid of Hermaan is now forgotten, and with it his treatment is involved in the same oblivion.

The analyses of the bile, which have been made in Europe, have detected nothing unusual in that fluid, and we discover no other remarkable changes in the other secretions of perspiration, urine, and saliva, than their increase or diminution, or some variations in the proportion of their constituent parts.

However this inquiry may be pursued, we shall find nothing to sustain the humoral pathologist. Even his very remedies discourage his philosophy. Does he endeavour to restore to the blood its fluidity by the transfusion of water, or recombine its constituent salts? He is, at most, elated only by the beam of hope, which for a moment animates the brow of his patient; but who soon relapses into the apathy of death, and brings a chill over the enthusiast.—That these experiments may continue to be repeated will be less remarkable, than the blind zeal of Hermaan, who successfully promulgated an hypothesis in Russia, which Ainslie had imagined and Annesley refuted in India.

The acid was fully demonstrated not to exist in the secretions, and it was no less shown that it could be only detected in its natural combinations in the blood. Has the saline treatment of William Stevens been attended with better success? There is good reason to believe, that even in tropical fever, which he esteems a kindred of Cholera, the principal agents were blood-letting to an extent of 30 or 60 ounces, calomel, emetics, sulphate of quinine and Croton oil.* It is useless to multiply objections. I know of no remedy, suggested by this hypothesis, that experience has sanctioned, or that has not left the projector its solitary advocate.

In conclusion of this subject, it is difficult to understand how the morbific poison can produce a direct influence on the blood, even if the vitality of that fluid be fully conceded. It is sufficiently difficult of apprehension, that the agency of the virus is originally through some part of the system, more material and more eminently endowed with the properties of life. That its primary action is exerted on the great source of sympathy and sensibility is even hypothetical, and although from the susceptibility of this system of organs to external impressions this is the most intelligible solution, it is still so impossible to comprehend the mode of action, that the mind involuntarily glances to other organs, or even to the unorganized blood, and seems to find repose in the greater darkness by which the subject becomes enveloped. If the poison be introduced and combined with the blood, as maintained by Searle, Stevens, Desruelle and others, the fact must be shown, before the doctrine can be tenable. Is the supposition that the poison produces its first impression on the solids more encumbered with difficulty, than that which introduces it into the blood through the medium of an organized part, and then supposes that in the course of its distribution, and thus diluted, it produces the physical changes through the system; or, than that which maintains that a direct and primary impression is made on the blood itself, by which the subject is not only embarrassed with the inexplicable mode in which that change is produced, but there is superadded an equal difficulty in explaining the possible manner in which the blood, thus diseased, so suddenly and so fatally deranges every function of life? The existence of the poison is absolutely hypothetical, and is only inferred from the necessity of supposing some universal cause, and from a long series of analogies; not from our ability to detect its presence in any mode of its existence. I need not therefore say, that the difficulty increases as we attempt to introduce an imaginary substance into the circulation, and that the design becomes utterly visionary, when we afterwards direct its influence to the organized system. Even the oxygen gas of the air, as stated by Stevens, is not "attracted into the circulation,"—nor even its azotic principle,—an assumption which was important to illustrate his doctrine.* If this negative fact prove any thing, it is opposed to the opinion of that gentleman.

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* Vide Medico-Chirurgical Review, vol. xx. 289.—My friend Dr. Holmes, of Montreal, informs me that he has tried Dr. Stevens' method in a few instances unsuccessfully. I have not known any useful results from it in New York.

It will be an interesting inquiry to ascertain whether the polypyform lymph observed by M. Marcus and his associates, and which is frequently noticed in this city, be sometimes produced before death, and how far it becomes a mechanical cause. I have witnessed it in subjects as early as five or six hours after dissolution, when the lymph was abundant and had acquired almost an organized appearance; and others assert that they have found it within the short space of half an hour.

Many enlightened pathologists ascribe the seat of Cholera to the *nervous system*, either without limitation, or restricting it to the nerves of the bas-ventre. Hence we have the doctrine of diminished nervous energy, and of central *ganglionite*, and *ganglionite néurilématic périphérique.* But are there any uniform indications that either system of nerves has sustained that impression, which is subsequently developed in the other great organs of life ; or any lesions of that system, through the whole progress of the disease, that bear any correspondence with the morbid phenomena of other organs ; or are there any symptoms which denote even a primary invasion of the nerves ? We have so little to direct us in this investigation, that the question will probably always remain undetermined, whether it be through the medium of this or some other tissue, that the poison deranges the general functions. But that the brain and nerves are not the primary seat of Cholera,—that they subsequently participate less than some other organs in the morbid action, and at most, do but serve to transmit the impression they may receive from the morbific agent to other parts of the system, I think may be nearly demonstrated. If we look at the mind, we shall find it "sitting unimpaired and serene amidst the ruins of organic life." Respiration is only performed by the voluntary muscles,—pulsation has long ceased in the extremities,—the heart has become inaudible to the stethescope,—yet the integrity of the mind remains undisturbed ; and the indifference with which it contemplates the wreck over which it presides, proves that at least its peculiar and last abode in the body is still its own uninvaded possession.

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* "I will afterwards," says Dr. Stevens, "bring forward some very strong facts to prove, that the aerial poisons which act as the remote cause of the essential fevers, do not produce their effect by any direct impression on the nervous system; on the contrary, they appear, like the oxygen of the air, to be attracted into the circulation, and produce their effects on the solids of the system, entirely through the medium of the blood."

The powers of the mind are fully exercised in respect to the voluntary muscles ; and it is not unusual to witness successful attempts at walking when the pulsations of the heart are only sensible to the ear. These phenomena are certainly not coincident with the attendant symptoms of other organs. The functions of the brain are wholly undisturbed, while the heart and the lungs, and all the viscera of organic life, are involved in a chaos of disordered action.

Animal sensibility is not known to be particularly affected, till near the termination of life. It is scarcely augmented or diminished, or in any way modified,—a remarkable circumstance, if we regard the common influence of disease on that property, and which serves to demonstrate the little participation of the nervous system in the great conflict of nature. The sufferer is even conscious of the unequal distribution of heat, and feels as intensely as in health, the action of stimulants upon the cold, and shrivelled and livid surface of his body. He hears and sees with a natural acuteness. The functions of sensation, therefore, remain unimpaired, and the great office of the cerebral system goes on to the last moment.

But these are facts with which all observers are familiar, and render the hypothesis of diminished nervous energy entirely paradoxical. The vital properties, with which the different parts of the system are endowed, are certainly not derived from the cerebral nerves; and the sympathetic system, as is imported by its very name, is chiefly destined to maintain a connexion between the complex parts of the most organized bodies. The various properties, by which the functions of organs are performed, exist in them independently. The analogies derived from vegetables, and the inferior animals, in proof of this fact, are too familiar to be repeated, and illustrate the peculiar office of the sympathetic system. Irritability, mobility, &c. are implanted in the organs which they are designed to govern, and where we find them peculiarly modified and adapted to the purposes of each individual part; and perhaps it is not improbable that the brain itself, in respect to its organic life, is sustained by properties quite independent of the nervous power. If the principles of life were imparted by the nerves, and there were really a diminution of nervous power, how does it happen that irritability, contractility, &c. are so exalted in some organs, that there are frequently mnifested evidences of congestion or inflammation, which have been assumed as the ground of a doctrine? I cannot but regard it, therefore, as an axiom in physiology, that vitality, which is made up of the several properties of life, is the specific endowment of every organ. The vague designation of nervous energy is wholly another subject, and has no necessary connexion with those powers which are so overwhelmed in Asphyxiated Cholera. That the nerves, in their office of maintaining internal, and perhaps also their external, relations, and transmitting sympathies, may convey in some mysterious manner morbific impressions to the properties of other organs, is more than probable; but it cannot be said, that vitality is affected till the poison has reached the seat of that principle. In what manner, then, does the morbific agent make its impression on the nerves, and is it through the specific irritability of that system, that that impression is transmitted to the powers or properties of other organs?

In respect to the ganglionic system, if we could attribute to it any other function, than that of establishing a circle of sympathies between the organs which it supplies, the hypothesis which assigns to it the proximate cause of Cholera must utterly fail from the want of pathological evidence. But from the fact that this intercommunication is maintained by the systems of nerves, and the simultaneousness with which the Cholera is often developed in the several viscera of organic life, I cannot doubt, that they are not only interested in the process as organs of sympathy, but have sustained an original part in the incubation of the disease.

All the doctrines, subordinate to this general principle of the failure of nervous power, must necessarily fall, if the original hypothesis cannot be maintained. We shall no longer understand how the "heart is paralyzed" if we look not beyond the nerves with which it is so slightly supplied. Irritability and muscular power are predominant properties of the organ, and its sensibility is almost latent. We do not find that the latter is in any way modified; but the former powers which belong to the constitution of the heart are most surprisingly disturbed. It is with this organ, as with the rest of the system; its properties and functions have become involved in the operation of the common predisposing cause; and perhaps it is less remarkable, that it should evince more of the morbid impression, from the eminent manner with which it is endowed with irritability, and from the general malignancy of the disease.

But my letter is running to an extraordinary length, and I have already presumed too much on your time,—and I fear, too, on the interest of my own speculations. I shall therefore proceed, in a few words, to express the opinion which I have been led to form of the nature and philosophy of this disease.

I believe that the proximate cause of Cholera Asphyxia consists in a *simultaneous modification of all the organic powers and functions*, produced by some unknown morbific poison, acting either directly on the properties, or transmitted indirectly through the nervous system. I am disposed to think, that the primary impression is on the organs of sensation and sympathy, and perhaps through the organic properties of that system. This change so produced is not merely a *depression* of the vital powers, but there is probably an *alteration* of their specific character, as seems to be denoted by the remarkable derangement of some of the functions. The constitutional disease has been sometimes so suddenly developed and so rapid in progress, that the powers of life have appeared to be at once extinguished.

That this modification is general and simultaneous will appear from the phenomena which distinguish the invasion. We must look at them, however, in the most simple and uncombined form of the disease; not as they occur in those complex cases that may be variously introduced by "premonitory symptoms." Those are instances in which either the epidemic poison, or accidental causes more or less frequently applied, produce some local derangement, generally of the organs of digestion, which subsequently becomes an exciting cause of the constitutional disease. Those are instances in which I hold, that there exists only local disease, which ultimately developes the specific action of Cholera. The exciting causes of those premonitory symptoms may also co-operate with the local disturbance, in producing the general affection; and indeed it frequently happens, that the predisposition to Cholera is so slightly formed, that the local disease subsides spontaneously, if its exciting causes be withheld, and that the general developement is only produced by an aggravated state of an almost artificial disease. That the miasm itself may produce derangement of particular organs is not opposed to the doctrine I advocate, as we have parallel instances in other epidemics.

These complicated cases of Cholera do not offer to the pathologist the unembarrassed data, which may be found in the less complex forms of the disease. If we assume a local proximate cause of the disease, we should always be able to detect its existence; but we have now been presented in New-York with many cases in which the invasion of the most malignant constitutional symptoms was abrupt, and in which there could not be detected a solitary evidence of antecedent local disease. I have known the attack to have taken place in the most robust subjects while sitting at their breakfast, in the apparent enjoyment of entire health; and I have seen them die within five or six hours after. The invasion has arisen from the strength of the predisposition, and these cases I believe to be always fatal.

Spasms are well known to occur extensively in some places during the prevalence of Cholera, without any other symptom denoting the epidemic influence; and at other times a suppression of urine has happened to many in certain endemics of this disease, without any other derangement of the health. As well might such a subject be considered a choleric, as he who suffers the "premonitory diarrhœa," and those local derangements might as well constitute the "first stage" of Asphyxiated Cholera. The transition from premonitory diarrhœa to the constitutional symptoms is certainly sudden in this city, however it may have been by slow degrees in other countries. The patient can often define the very moment of the constitutional invasion, and the precision with which it is done seems alone to distinguish the diseases.

There is nothing in this simple form of the disease that denotes an original locality; and if in its progress, or even at its invasion, we find one organ sustaining more of the burthen than another, and vice versa, it is only what is constantly witnessed in other constitutional affections, and does but arise from their varying susceptibilities, or the idiosyncrasies or other accidents appertaining to the subject.

It follows from the principles which I have endeavoured to advocate, that the extreme vessels must be the instruments of this disease, and it is upon those organs that we must make an impression in the treatment. One of the morbid conditions is a general contraction over the whole body. This will explain some of the vital, and some of the more mechanical phenomena. It will account in part at least for the carbonization of the blood, which can no longer pursue its unembarrassed course through the lungs; it will account in part also, for the shrinking of the skin, and the collapse of the features. Such a universal contraction must affect the general circulation and contribute to the venous congestion, and probably produces a strong impression on the heart through the medium of sympathy. But doubtless more is owing to the nature of the morbid action itself. It is established in every organ; in the liver and kidneys it is especially denoted by the failure of their secretions. It is a useless multiplication of causes to ascribe the deficiency of bile to the failure of digestive excitement;—and its disappearance from the evacuations has been unnecessarily assigned to a contraction of the ducts from irritation of the intestinal mucous membrane. We constantly find nearly the same quantities of bile in the vesicle after death from other diseases, and it is not usual in Cholera for that organ to be distended;—nor have I ever been able to detect more than a trace of bile in the liver, and often none. If we generally find no urine remaining, it is because the bladder is a voluntary organ, and participates also in the muscular spasm. The kidneys being more isolated, have been permitted to take their full share in the constitutional affection. So also has the heart, and the remarkable flabbiness of this organ, which I have frequently witnessed, denotes something more than sympathetic derangement; though I cannot doubt that its embarrassed action is owing also to the influence of sympathy from different sources.

Perhaps there is no symptom that more strongly evinces the constitutional derangement than the failure in the production of heat. This is one of the earliest, and most general phenomena, and exists in every part, excepting the epigastric region, where it sometimes accumulates in a morbid and remarkable degree. The coldness of the breath and extremities, the generation of heat in the vicinity of the liver and stomach, and its distribution from this part over the system, after the laws of vitality cease to operate, are proofs of its production, by a vital process, and that its organs of secretion are universally involved in disease.

In reverting once more to that plausible theory which refers the proximate cause of Cholera Asphyxia to the intestinal canal, and which is necessary to its identity with cholera morbus, I cannot but conclude, for the reasons I have stated, that there is little evidence that the local derangement is the primary result of the action of the morbific poison as constituting the general disease, whether we advert to the symptoms, or repose the decision on morbid appearances. In the succession of phenomena which mark the invasion or the progress of Cholera, we sometimes notice other symptoms long anterior to any evidence of the extension of the disease to the bowels, or we find simultaneous disturbances in other organs not less remarkable: Dissections are no more conclusive as to the proximate cause, and it would be idle to repeat the results which establish the want of that uniformity which can only sustain an important principle ; results, which might with nearly equal propriety, refer, indiscriminately to any of the great organs of life, the seat of the disease. This variety of local developements alone carries us to the true philosophy of the disease ; and when it shall be established that the whole system is simultaneously impressed by the morbific poison, then shall we understand why one organ, more susceptible in one individual than in another, or more subject to the influence of those sympathies by which it is connected with the whole system, and vice versa, is found sustaining more or less than a proportion of the morbid action, and in its necroscopic character supplying corresponding evidence of disease. Then may we comprehend the greater coincidence in the vital phenomena, than in the morbid appearances; and can better understand the rapid destruction of the powers of life where the whole system participates in the invasion, than by referring these astonishing events to the influence of disease as established in a single organ.

If what has been denominated "premonitory diarrhœa" be regarded only as the result of some local affection, and ultimately becoming an exciting cause of the constitutional derangement,—then will be explained the mystery which has enveloped the issue of the treatment, when applied under circumstances of local or general disease;—then may we comprehend how abstinence alone, when diarrhœa portends the danger, will enable nature to effect her escape,—or how it happens when neglected for a day or an hour, it becomes the exciting cause of a perilous disease, which had been but the preceding moment in a state of incubation, and which now baffles the efforts both of nature, and of art;—or if submissive in cases of rare felicity, its successive and capricious developements still require the vigilance of zeal, and the decision of a firm observer, to conduct it to a slow and successful issue.

My construction of the antecedent diarrhœa is rendered still more probable from its general absence in Asia, a circumstance particularly noticed by some of the oriental writers. If it be true that it has occurred more remarkably in Europe and America, as a premonitory symptom, it only furnishes an evidence that there exists some modification of the predisposing cause, by which a local disease is produced before the system becomes sufficiently imbued with the poison for a spontaneous developement of Cholera. The disturbances which arise in the alimentary canal, as attendant on the constitutional disease, have been every where observed, with isolated exceptions, and they have been among the primary symptoms of Asphyxia. We hence infer that the choleric atmosphere produces a particular determination of disease to those organs; and from the impaired digestion which is sustained by most, during the prevalence of the epidemic, it is not difficult to comprehend a circumstance which is strictly analogous to the phenomena of other epidemics,—that the malaria may produce in one a depression or change in the gastric powers,—and in another some functional derangement of the bowels, without exerting its malign impression on the whole system; or, if its influence be so extensive, the local affection becomes rather an exciting cause of the more general and independent disease, than existing as an integral part, until the general developement.

I regard the Cholera Asphyxia as a *nova pestis* in Europe and America, and only known in Asia within the last half century. I cannot recognise its parallel in ancient or modern authors. I consider it a *fever,* of which the *collapse* is the first stage, and *reaction* the second. The *local determinations* which arise in the second stage are not necessary to the disease, but are accidental.

Very respectfully, &c.

LETTER VI.

New-York, *September 10th,* 1832.

Dear Sir,

Having submitted to your consideration my reasons for believing the Cholera Asphyxia to be a constitutional disease, I would present you in this letter with a brief analysis of the Epidemic as it has occurred in this city. I have already stated the circumstances under which the irruption took place, and without adverting to what may be regarded as its statistics, I shall proceed at once to detail its phenomena, and shall begin with

A *general description* of the disease. The disease first presented itself on the 27th of June, and attained its height about the 20th of July; after which it declined till the first of September, when it again increased, though not extensively. It has long been remarked in other countries, that the Cholera makes its attack in the night, and particularly towards the approach of day. I think it has so happened in New-York in a majority of cases; and the next period most common is in the morning soon after breakfast, and the next in the afternoon. I allude, of course, to the accession of the malignant symptoms, which is generally almost instantaneous. The patient is suddenly disturbed with a sense of nausea, and oppression near the region of the stomach, or of exhaustion or vacuity at that organ, and there immediately follows a movement or a commotion of the bowels. A feeling of coldness invades the whole body, though rarely exceeding chilliness. Vomiting and purging now occur simultaneously, and if diarrhœa had previously existed, the evacuations from the bowels exhibit the appearance of rice water, in which is diffused a moderate portion of flocculent matter, that soon subsides to the bottom of the vessel, leaving the fluid more like the appearance of common water. The nature of the first ejections from the stomach will depend on the ingesta that may remain in that organ, and subsequently they assume the appearance of the dejections. This intestinal commotion is attended with various degrees of pain or distress, or sometimes there only arises a feeling of exhaustion, and the prostrating effect from vomiting. An insatiate thirst ensues, which becomes more intense and annoying as the disease advances. The next symptoms in the order of succession, are a general languor, and incipient listlessness. The pulse begins to contract, and increase in frequency. The skin is losing its natural warmth, the nose becomes cold, and the patient either complains of increasing rigor, or admits its existence. He now seeks repose, often feeling some vertigo, or replying that he hears a buzzing, or noise. The abdominal muscles are found contracted, and sometimes feel like a board, though no spasm exist. At other times they are doughy or are natural. A sense of tenderness is sometimes, but not severely felt, and the abdomen may generally be pressed without producing uneasiness. Locomotion is impaired, but the patient is often able to walk till near the extinction of life. Spasms soon begin to ensue, affecting particularly the muscles of the feet and toes, but often the corresponding muscles of the upper extremities, and sometimes those of the body, though more rarely the muscles of respiration. The pulse now becomes smaller and more accelerated. Perspiration breaks out on the skin, and every effort at vomiting increases its profusion. The temperature continues to sink, and the thermometer now evinces a general interruption of the calorific function. Restlessness increases, and jactation begins. The skin is becoming corrugated around the fingers and hands, and the same appearance is often remarked on the feet and toes. This symptom frequently increases till it exhibits the appearance of great emaciation, the skin lapping in folds. The countenance, which had become anxious, now assumes a pathognomonic expression. The features contract, but are less hippocratic than observed in the East. The eye partially retreats, and the nose is pinched. Listlessness has now grown into indifference, and the attention of the patient is only arrested by suffering or by an inquiry. Abdominal distress increases or supervenes. Respiration becomes more and more embarrassed, and sighing comes in to its relief. The countenance assumes a uniform leaden hue, or extravasation of blood converts it to an African complexion. The skin preserves its sensibility, but its irritability is almost extinguished. The patient is often aware of its coldness, though he frequently complains of heat, and stimulants applied externally will often act as soon on his sensibility as in health: But not so the irritability; there is no inflammation from the most potent of the ordinary applications, and it is even with difficulty that any vascular excitement can be produced. The thirst which existed from an early period, is now importunate, and induces more complaint than any other species of suffering. The tongue is generally coated in the centre, and clean at the tip and edges, and almost constantly moist, and becomes colder and colder as death approaches. The breath announces the same subsidence of internal heat, and the hand is chilled on the extremities, while it sometimes burns when applied to the region of the epigastrium. If this language be strong, it is intended to convey a sense of the contrast which can be in no other way imparted. The voice becomes guttural and feeble, or is depressed to a whisper. The secretion of urine has been suspended from the invasion, and the patient will sometimes advert to the circumstance when far advanced in the stage of collapse. If the spasms continue, he will sometimes insist on walking for his relief, when every other symptom announces the near extinction of organic life. If blood have been taken after a full developement of the disease, it is unusually dark and is obtained with difficulty. The pulse becomes tremulous,—it is no longer felt,—the action of the heart is only known by the stethescope,—respiration is performed by the voluntary muscles, and yet the mind tranquil and almost undisturbed in its functions, till absolute death severs the connexion.

These phenomena represent the common progress of Cholera Asphyxia when it advances without interruption to a fatal termination. It will be seen that they are nearly parallel with those which have characterized the disease in Europe and Asia. Some of the symptoms have been, doubtless, modified by climate and other accidents, which it will be the remaining object of this letter to illustrate ; but their coincidence is so remarkably striking, that we contemplate the disease with the greater astonishment. In the early part of September, the Cholera, after having subsided for several days to some 20 or 30 cases, again increased, and became more sudden in its invasion. It was often unaccompanied by any of the former "premonitory symptoms," but its victims were frequently struck down at a blow, and became immediately pulseless. Such patients were unsusceptible of any action from remedies, and generally perished in a few hours. They afforded examples of the true and unmixed character of the disease, and very distinctly illustrated its identity with the Asiatic epidemic. Jactation, and oppression at the præcordia, were also remarkable symptoms in such cases. There was no tenderness of the abdomen, and but little or no pain accompanied the purging.

When the second stage, or that of reaction took place, there was a simultaneous abatement of the most appalling symptoms. The pulse began to rise and the features to expand. Purging and vomiting had ceased. The production of heat was again taking place; the urine began to flow; and if local disease did not supervene, the phenomena indicated returning health rather than a violent conflict of nature. Still the recovery was slow; and the increasing fur on the tongue, the morbid state of the secretions and their gradual improvement, denoted a recovery from some severe convulsion.

The varieties which have occurred in the symptoms of Cholera in this city, have been less remarkable than such as were noticed in Asia. The prominent features of the disease have been more uniform, though some of them have slightly varied from the epidemic of the East. The collapse of the features has been less remarkable here, and very frequently absent; and so also has been the lividness of the countenance, although that was less frequent in Asia than commonly supposed. The absence of vomiting or purging, or both conjointly, was more frequent in the East, and spasms are less violent and more partial here, and more easily subdued. I may add, too, that our disease is much more fatal.

The developement of the constitutional symptoms is generally sudden here, as it was in Asia;

but it is almost constantly preceded by diarrhœa, which seems to be only connected with the predisposition to the general disease, as it may have proceeded from a common predisposing cause. But this diarrhœa is evidently very often of a more artificial nature, and owing much to the habits of the individual. I have hitherto stated to you my opinion of its local character, and that it is in no respect a symptom of Cholera, till it may have become an exciting cause of the constitutional disease; and till then, that it is susceptible of easy control. The discharges attendant on this preliminary state of the bowels are generally different from those which are considered almost characteristic of Cholera, and denote a different mode of action. They are also usually coloured with bile, and if they become watery, it is a circumstance which invariably attends common diarrhœa of considerable duration. If this preliminary diarrhœa, therefore, was rarely observed in the East, its absence does not affect the very great identity of the disease. It was a prominent symptom of the collapsed Cholera, and the evacuations at once exhibited the failure of the bile, which is not often noticed among us till the same general condition of the system supervenes; a circumstance which appears rather to favour the belief of an entire coincidence. It happens, however, that this premonitory diarrhœa is sometimes complicated with an antecedent disease, either functional or organic, and may be consecutive or accidental. When the previous derangement has been merely functional, it requires a close investigation to determine how much is to be ascribed to the predisposing cause of Cholera; but the history of the case may disclose some previous affection of the digestive powers,—some congested state of the liver,—or some other derangement almost unobserved from its slow accession, but now aggravated by a choleric atmosphere, and has become the source of some sympathies, which may be mistaken as indicative of the predisposition that soon results in Asphyxiated Cholera. Again, very nearly the same phenomena will proceed from the malign constitution of the air, and are essentially connected with the formation of the specific disease. An attention to the case will enable us to distinguish the causes and the nature of these morbid conditions. To confound them is sometimes fatal to the patient, and it is better to give them the worst construction when any doubt may remain. I have repeatedly witnessed, during the prevalence of Cholera, congestions of the liver and some corresponding affection of the mucous membrane of the stomach and bowels, which evidently proceeded from some previous dyspeptic malady, or from the influence of hidden causes, existing perhaps in the atmosphere. These cases have been more or less distinguished by a slow or accelerated pulse, sometimes slow and contracted, and again full and quick and frequent, and in either case sometimes intermittent or otherwise irregular. Corresponding conditions of the skin have been attendant. In the former instance, it has been cool and rather dry ; in the latter warm or hot, moist or husky. The urine has been deficient, red and irritating. The head painful, or a tightness of the brow, and soreness and stiffness of the eyes. The countenance pale and sallow, or flushed and inanimate. The tongue coated in various degrees, generally more extensively than in Cholera, and commonly of a yellowish tinge, or a dirty white. The mouth rather dry to the patient, and the taste altered. Indefinite pains occur in the limbs and the body. There is often some tenderness and throbbing at the region of the stomach, or a sense of exhaustion from pressing on that organ. The appetite is impaired, and a little food produces fulness. The bowels are constipated, and cathartics are of slow and difficult operation, unless preceded by ample blood-letting. The dejections are rather black, or green, and intermixed with mucus ; or if salts, or senna, have been given at an early period, the evacuations were watery, and might be mistaken for rice water. The uniformity of temperature is often disturbed, the feet being unpleasantly cold. Muscular strength is variously impaired, and the patient seeks rest and retirement. His spirits are depressed, and he becomes impatient for relief.

Such is the most common form of disease next to diarrhœa, that has occurred to my observation during the prevalence of Cholera. It readily, though slowly, yields to our remedies, and we have therefore no opportunity of seeing it degenerate into Asphyxia. It is constantly seen in this climate, and has not been at any time excluded. I am sensible it has been regarded as a part of the reigning epidemic; but my conviction is opposed to that opinion. That it may become the exciting cause of Asphyxiated Cholera is undoubtedly true; and that symptoms considerably analogous constitute the accession of that disease, I think I have sometimes observed. They are, however, of brief duration, and are scarcely known to the physician, but by information from the friends or the sufferer. Collapse has supervened, and we commonly meet the case presenting the phenomena I have described.

The symptom by which the disease is most distinguished, and from which it has derived its specific name, is invariably attendant, and occurs under the same modifications which have been noticed abroad. The pulse generally becomes contracted and accelerated soon after the invasion, and if the disease be not complicated with previous inflammation, it is never hard. It is especially influenced by vomiting and spasms, and by sighing, and by voluntary motion. It is more remarkably depressed by the former; becoming even imperceptible, as is frequently observed in the spasms of hysteria, than increased in volume by the latter. As these causes cease to operate, the pulse again resumes its character. Its frequency increases as the disease advances, and there is a corresponding diminution of its volume. These phenomena, in a majority of cases, go on in a progressive ratio, till the pulse can be no longer distinguished in the extremities. At other times it becomes irregular, though still frequent, and its motion is often tremulous, and the pulsations cannot be counted. If the ear be now applied to the region of the heart, its action may be found to be perfectly distinct, and not exceeding a hundred and thirty or forty pulsations. This want of synchronism is remarkably striking in some subjects, and when it exists, the pulse sometimes becomes obedient to the action of the heart, as the disease marches on. It does not seem to arise from any accidental occurrences, but is obviously a contingent symptom of the disease, and I believe invariably a fatal one. Again, the pulse is not remarkably affected at the invasion, and only assumes the choleric character after a full developement of the other symptoms. The average frequency of the pulsations from the time of the attack, and while the pulse remains distinct in the extremities, is from 100 to 130. The pulse is rarely intermittent, as distinguished from irregular. It may be commonly felt in a very attenuated state till near the extinction of life ; or sometimes it wholly disappears, and is reproduced by artificial means, or returns without any apparent cause. It had rarely happened, however, that the patient lingered beyond three or four hours after all pulsation has ceased in the radial artery, until a modified type of the disease was observed after the first of September. I had known only two parallel instances to those recorded by the Anglo-Asiatics, in which death did not supervene till two or three days after the disappearance of the pulse; in one of which, the patient lingered four days without pulsation at the wrist, and the other, three days. I have since seen two similar cases, through the politeness of Dr. Lee; in one of which, a very respectable lady of robust health became pulseless soon after the attack on the 6th of September, and remained so for 58 hours. The temperature of her mouth was 96 degrees for 30 hours after pulsation had ceased. She also passed urine involuntarily several times during a period of more than 38 hours of perfect Asphyxia. She had no sweating, and her tongue was nearly clean, though rough and rather florid. The purging had been profuse, no vomiting, and spasms very slight. Her skin was leaden and warm, thirst intense, and countenance anxious. The pulsations of the heart were 136, thirty hours after the symptoms of Asphyxia had taken place. During all that period she was perfectly intelligent and very colloquial, conversing in a most interesting manner. Her voice was hoarse and unnatural. Thirty-eight hours after Asphyxia, the temperature of her mouth stood at 98 degrees, and the pulsations of the heart were 140. At fifty hours the temperature was nearly the same, body warm, extremities cool, surface livid, features not collapsed, no wrinkling of the skin, no sweating, purging continuing, action of the heart more accelerated, no sleep since Asphyxia, sordes collecting on the teeth, and delirium had existed for twelve hours. Was still able to jump from her bed. Respiration nearly natural, and breath warm. At fifty-eight hours, respiration laborious, action of the heart inaudible, temperature of the mouth 90 degrees; over the entire hypochondriac regions the thermometer stood at 106 degrees, the temperature of the room being about 74 degrees. Extremities cool, not cold, comatose. Skin of a deep leaden colour. Passed her water at fifty hours, but whether afterwards is not known. As late as fifty-three hours after the cessation of the pulse, it required considerable strength to confine her to the bed. She died twenty minutes after the last record of her symptoms.

The case was treated principally by calomel and Dover's powder. Fatigue, and a cathartic taken without advice, were the exciting causes.

I shall subjoin another of these remarkable exceptions from the ordinary form of Asphyxiated Cholera, which fell under my observation.

Mr. A., a patient of Dr. Lee's, resided at No. 142 Christopher-street, a respectable butcher of prudent and temperate habits, aged about 50 years. He was seen by Dr. Lee at 10 o'clock of the morning of the 7th of September. He had been affected with diarrhœa during the summer, and was attacked with collapsed Cholera on the night of the 6th. The discharges suddenly became profuse, and were unattended with pain. There had been no vomiting, nor did that symptom, or even nausea take place at any subsequent time. No chilliness, nor muscular pain, nor much oppression. Dr. Lee found him without pulse, and the temperature of his body natural. I saw him first with the Doctor at 6 o'clock on the evening of the 8th, at which time his symptoms were said to be the same as on the morning of the preceding day. His countenance denoted nothing but health, save a vacancy of the eye. It was covered with a natural flush, and the cheeks were pleasantly warm. A slight shade of blueness appeared on the body, and extremities, which were but little cooler than natural. The fingers were assuming the square form, but were without wrinkles. The veins collapsed and the skin lost its elasticity. No perspiration, nor any unnatural dryness of the skin. Temperature of the body and extremities but little lower than natural. In the mouth the thermometer stood at 95 degrees. The pulse entirely insensible, and the action of the heart very feeble; its pulsations 92 in a minute. Had one slight spasm in the leg the day previous. Passed his urine frequently and freely, of a yellowish colour and generally without volition. Purging was arrested the preceding day, and had scarcely returned. The dejections were watery and without smell. No tenderness of the abdomen, no sense of oppression, but little thirst, drank moderately, but ad libitum, and without inconvenience. Tongue moist, slightly coated with a whitish mucus, and rather pale. Muscular strength considerably impaired, and at this time, he required assistance in turning. Voice altered from the time of the invasion, being hollow and feeble. Spoke with effort and reluctance. Intellect entire, but great listlessness and indifference.

He died at 2 o'clock on the following night, without any return of purging, or any suffering, having been without pulsation 40 hours from the first visit of his physician. Eight hours after death, the temperature of his mouth was 78 degrees, and over the epigastrium 84, that of the room being 72 degrees. His skin was not livid, and there was still a slight flush of the face.

Calomel was the principal remedy.

After the 1st of September the Cholera again extended and increased in malignancy; I had at no time known it to present a more formidable aspect, and it has occurred in many instances without any "premonitory symptoms," and sometimes without any apparent exciting cause. From that period, I have seen the pulse extinguished, in many cases, at the very invasion, and not again returning, the patients expiring in four to twelve hours after.

The superficial veins are flattened from an early period, and the blood, which flows from them in bleeding, seems not to be restored, or but very gradually. Hence it is nearly impossible to obtain much blood, in this manner, from a collapsed subject. It runs freely for a short time only from the temporal artery, and is obtained with great difficulty by cupping and leeching. In

Asia large quantities of blood were often abstracted from the veins of collapsed subjects, but always with difficulty. This symptom will not affect the identity of the disease, if we advert to the far greater integrity of the blood in the East; and the infrequency of premonitory diarrhœa. It is always particularly dark from the veins, and in consistence more than naturally thick. These phenomena are observed to increase as the disease advances, and, I believe, they are uniformly present. I have heard of no instance in which the blood flowed readily, or in which it was not in some degree discoloured, at whatever period abstracted. It has very generally happened that the blood has coagulated slowly and imperfectly, and often not separating into its component parts, unless drawn into a vessel best adapted to promote that investigation. The coagulation, however, will be soonest and most perfect as the blood may have been abstracted near the time of the invasion, and it is liable to be influenced by previous diarrhœa, &c. It is deprived of its serum in proportion to the duration of the disease, and the premonitory diarrhœa, and this appearance occurs where the purging and vomiting may have been very slight. In those cases the watery portion had escaped by the skin, from which the perspiration runs almost in a current. However carefully abstracted, there is no buffiness, or scarcely a pellicle of lymph on the surface. In the East, the blood coagulated quickly, and the difference is easily explained without affecting the identity of the disease. The evacuations were less profuse, and there having been little of our antecedent diarrhœa, the blood was less exhausted of its serum, and some other component parts,—a circumstance which must materially affect the sensible phenomena of that fluid. Thus, too, is explained the greater success which the Asiatics derived from blood-letting, than has been enjoyed in Europe or America. The remedy was there applied before the blood was bereft of all but its crassamentum, which has been with us but the poor remnant on which despair has sometimes seized; or if in a few solitary cases, the fluids have yet remained in the system, and some determined hand, well directed by the lights of science, has promptly liberated the circulation, it exemplifies the feeble assistance which is afforded by nature when she has thrown off the principles of the blood, and affords another analogy between our own and the Eastern epidemic. In these rare cases, I am told, what might have been expected, that the blood soon becomes more florid, and flows more and more freely.

The colour of the blood does not appear to be much influenced by the manner in which respiration is performed. The mechanical part of this function often appears to go on well after pulsation has ceased, and the skin become livid. Neither do the secretions affect the colour, which appears to depend on the period of the collapse.

I cannot doubt, that the colour results in a great degree from some interruption in the organic functions of the lungs, and to which I adverted in a former letter.

Dr. Gale, who kindly favoured me some time since with a few results of his experiments on the blood, and which are noticed in one of my former letters, has more recently detected the existence of an oil in a free state in every part of the vascular system, and which I have since had opportunities of witnessing in dissections. It is sometimes very conspicuous, and resembles olive oil. It abounds most as the patient approaches death.

The difficulty with which the blood is abstracted from the veins is never owing, directly, to its thickness, but to the interruption in the circulation. Its consistence, although obviously increased, has probably been overrated by authors.

The varieties which occur in the *Purging* are not numerous. Notwithstanding this evacuation has been abundant during the antecedent diarrhœa, it becomes still more profuse when Cholera is developed. The quantity is sometimes surprisingly large, amounting to more than fifteen quarts in twelve hours. This is probably the largest estimate, and in such instances there has been comparatively little vomiting or perspiration. Some rare cases have occurred in which this symptom and vomiting have been absent. The discharge takes place without much, if any, pain, and is unattended with effort. In a plurality of cases it occurs without volition, after collapse is fully formed, and does not attract the notice of the patient, though at an earlier period the impulse may have been strongly and irresistbly felt. The fluid is ejected very variously, often with violence, rarely appearing like the action of cathartics. The patient sits for a moment on the pan, and we are astonished when he rises, to observe the quantity discharged. Again it runs away slowly, or there is merely an oozing. It is attended by an expression of great exhaustion, and is felt by the pulse. It continues, if not arrested by art, till near the close of life. The sensible properties of the fluid evacuated are such, almost uniformly, as I have described. Sometimes it resembles dirty water, and more rarely presents a bluish appearance. It occasionally exhibits more of a milkiness, than of the rice water; but in that case a precipitate takes place, and leaves a colourless serum. I have seen two fatal instances in which a deep greenish yellow bile was vomited and purged till near the extinction of life. The surface was livid and the pulse as usual in Cholera. They are very rare exceptions. In a few instances at the hospitals, there has been seen the reddish appearance like the washings of beef, as described by authors. In these cases there is more abdominal suffering. The discharges from the stomach and bowels are nearly alike. The dejections are generally inodorous, but are very rarely attended by a strong and offensive smell. Excessive discharges constitute a hopeless case, and the most malignant have been now and then unattended by any. They are generally easily arrested by appropriate means, and consequently it does not long remain a symptom after medical treatment is applied. From the abundance of fluid, however, which is frequently found in the canal, I have no doubt that the secretion often goes on, notwithstanding its discharge has been arrested, and continues to exert an influence on some of the other phenomena. The discharges are occasionally the obvious result of affections of the mind. Fear, particularly, will suddenly excite profuse evacuations; but how far its action extends to the organic functions is doubtful. The secretion may have been already produced, and the muscles alone have felt the impulse. It need not be said that purging is an earlier symptom, and is entirely more excessive and uniform than vomiting. I do not know that it has ever been absent where vomiting has appeared.

In India this symptom occurred only during the collapse; it was then excessive and of short duration. In New-York diarrhœa has generally pursued the patient for some hours or days, before the constitutional disease is developed; then becoming even more severe than at the East, and continuing with greater pertinacity until arrested by art.

Vomiting is comparatively infrequent with the purging, but occurs early among the symptoms of collapse. It often subsides spontaneously, especially if the stomach be not irritated by drink. It aggravates the thirst, but is not connected with its production, so that no relief can be obtained from fluids. The ejection is not attended with much pain, but is often effected with severe efforts. The ingesta, however, are not always entirely thrown off. It never occurs without purging, and is much more easily arrested. It is always aggravated by cold or hot drinks, unless in very small quantities, and affections of the mind will produce it. It is also protracted and rendered more obstinate by emetics. It is very frequently wanting, and cannot be regarded as a severe symptom of the disease. I know not that the case is more favourable from its absence. The fluid is sometimes discharged in large quantities, and generally resembles the dejections. Again it is in small quantities, and thrown up with greater effort, being more like the natural mucus of the stomach. It is not infrequently tinged with green, sometimes bluish and very rarely yellow. I have seen it ejected in large quantities of a deep greenish yellow, when the patients were far advanced in the worst varieties of collapse. The exhaustion from vomiting is greater, but less permanent and extensive than from purging. It does not appear to affect the state of the skin in respect to perspiration. It seems not to be influenced by temperament, or sex, but is evidently more connected with the habits of the individual. I have seen it occurring during the violence of the epidemic, as a symptom of our indigenous cholera morbus. It was then more obstinate, and attended with bilious discharges. The diseases presented striking dissimilarities.

The most remarkable condition of the *skin* is the impaired state of its *irritability*, while its sensibility is not much diminished. Simple irritants, however active, do not produce their ordinary effect. Cantharides, ammonia, &c. are perfectly powerless, and the chemist is consulted for combinations that shall evolve more active principles. These, too, fail in their turn, and we resort to fire and boiling water as the last resource of art. But the patient will not even always burn —save in his sensibility, which pleads as piteously as in the fulness of health. The mustard cataplasm soon becomes a source of great annoyance, and Rhinelander's ointment sometimes produces expressions of great suffering. Still the marble coldness remains, and vascular action is as dead as ever. There is something characteristically striking in this state of the temperature. It is the coldness of death, and if the skin be wet with perspiration, a chill is imparted to the touch. This absence of heat is often confined to the extremities and face, though generally existing in an inferior degree over the greater part of the body. It is frequently preternaturally great at the scrobiculus cordis, where I have found it accumulated in a dying choleric at 106 degrees. I am informed that in two cases at the Greenwich Hospital, the heat of the skin was almost natural till death. In those instances the pulse was very indistinct during collapse, and there was no sweating. In the case of the lady to which I have alluded, the temperature of the whole body was preserved without a parallel. I have never remarked an increase of the temperature as the extinction of life approaches, excepting at the region of the epigastrium ; where it sometimes acquires a great exaltation just before death ; but after death the general surface often becomes warmer. I ascribe this circumstance, not to the production of heat, but to the cessation of the vital laws, which enables those that govern dead matter to operate, and to equalize the caloric already existing. The coldness of the skin is increased by sweating, but is less so than would be inferred, *à priori*. It is also increased by purging, but not in a remarkable degree. In adverting again to the loss of irritability, we are sometimes astonished that boiling water will produce no vesication, when it is known to vesicate other subjects if applied immediately after death. It would be folly to contend that the living man is more dead, than he who is defunct beyond all question. This phenomenon can be only explained by recognising a peculiar morbid condition of the skin, in which its organic properties are so changed, that they have entirely lost their natural susceptibilities ; and it is another argument against the principle of sympathy, and in behalf of constitutional disease.

Sweating has been a very variable symptom: At the irruption of the epidemic, it was most universal, and became less uniform after its decline.

From the 1st of September it was absent in a majority of cases, and less frequently attended those subjects who were pulseless from the attack. It occurred in various degrees, from the most profuse, to an insensible transpiration, the skin being rarely preternaturally dry. It was always cold, and rather clammy; but in subjects transfused, it was suddenly increased in quantity, and its temperature exalted to a hundred degrees. Its odour was strong, and so very characteristic, that the disease could be known by the smell. It was most abundant when purging was least, but they occurred in connexion to an excessive degree.

The skin, in all subjects, lost its *elasticity*, and if pinched, would slowly resume its former condition. A remarkable appearance was its corrugation and folds on the fingers and toes. It occurred under every aspect of the disease, and was more or less present in a great majority of cases. It was seen in subjects where purging and sweating were not severe, but most so, where the latter was excessive, and it often happened in the entire absence of sensible perspiration. It was less conspicuous on the toes than the fingers, and often involved the entire hands and feet. A flattening of the four sides of the fingers was first observed, producing a squareness of shape. The folds then succeeded, longitudinally on the front and sides, and tranversely on the back; and if they became numerous and large, the squareness of the fingers became less distinct. If sweating were profuse, the fingers and toes were greatly reduced in size, and the skin was remarkably corrugated. This corrugation appeared to arise from three causes; in part from the direct morbid action of the skin, from sweating, and from other evacuations. The limbs did not exhibit an appearance of emaciation.

The *colour* of the skin was less uniform than many other symptoms, but was affected in a majority of subjects. The prevailing hue was leaden, and like other varieties, increased with the progress of the disease. It was not commonly most obvious about the eye, as has been elsewhere noticed, but existed in an unvarying shade over the surface. If there were congeries of varicose veins, they exhibited dark patches. This tint, more than any other, increased the cadaverous expression. Next in frequency were various shades of blueness, and this occurred in most instances of sweating, the leaden colour being more peculiar to a dryer state of the skin. It abounded most on the nails, hands, feet and face, particularly the lips, but did not especially exist around the eyes. It was also diffused in a lighter shade over the body, and was remarkable on the contracted scrotum. When the sweating was excessive, the hands would sometimes lose the colour and become blanched, especially the palms, the nails remaining blue. A still darker hue was frequently observed, occurring on the body and limbs in large and small patches, and covering the face entirely or partially. It would sometimes recede, and again appear, and was often influenced by the spasms. It would sometimes subside before death, and return in a short time after ; or it would disappear soon after death, and reappear on the upright part of the body within an hour or two. A still darker hue, approaching the African colour, was more rare and owing to extravasated blood. I have seen it covering nearly the entire surface of the body.—It was connected with cases of great malignancy, and in which the spasms were violent. A deep brown and a leaden colour frequently occurred around the eyes, but when it was distinct there, it was scarcely observed on the face. In a large proportion of cases, there was no striking change of colour, and in some instances of most perfect Asphyxia, the countenance was florid as in health. The nails were often curved, when the fingers were much shriveled.

The *countenance* of a choleric can seldom be mistaken, notwithstanding the variety by which it is distinguished. It is not, however, generally, the cholera countenance of the Asiatics ; and perhaps in no feature of the disease does there exist a greater disparity. At the irruption of the epidemic I saw many cases presenting the true collapsed, shrunken features, described by oriental writers, but they have been less common since. They were attended by the whole array of the worst symptoms. In most instances there occurs some sinking of the features, particularly of the eyes, the expression of which is differently affected, being generally dull, and very rarely presenting the dim and opaque appearance that was so constantly noticed in the East. This organ is fixed, and moves only when excited by some mental or physical impression; but seems as ever sensible to the light, of which the patient sometimes complains. There is scarcely any winking, and thus far the combination represents the inanimate statue, which derives further effect from the stillness of all the facial muscles. If the patient sleep, it is with his eyes nearly open, and the lips parted. Sometimes the eye acquires an unnatural lustre, but with the same appearance of vacuity; and this will remain a very striking object for two or three hours after death,—slowly fading away. Such cases are attended by severe spasms. The pupil exhibits every variety of contraction and dilatation. Petechiæ sometimes appear on the sclerotic coat. I have never seen the "cornea apparently covered with a film."

In a majority of subjects the cheeks are rather moderately sunken than collapsed,—rarely as much so as in bad cases of cholera morbus. The nose is more pinched, and especially so when the alæ are concerned in the respiration.

Again, there is scarcely a perceptible sinking of any of the features. They are, however, all quiet, and the inanimate eye is sufficiently characteristic. Rarely, there is a natural flush, and an expression of health, and nothing to denote the near extinction of life, save the never failing spiritless eye. The expression evidently depends in a degree on the intellect and habits of the individual.

A particular coincidence has occurred between the *spasmodic affections* of our cholera subjects and those of the East. It is with us, however, a more common symptom, existing in at least three fourths of the cases: Of all the symptoms it seems to denote more than any other a primary impression of the morbific agent on the nervous system, and that its influence is thus, in part at least, propagated to other organs. It is, however, but an isolated evidence, and rests on its frequent occurrence as an early symptom of Cholera, and the fact that it existed merely as a symptom in many people in the enjoyment of health, especially during the increase of the epidemic. It was sometimes late in its appearance, and was entirely most violent, where the purging and vomiting were most severe. It was most so in the robust, especially if intemperate,—and more so in males than females. In children it was rare or slight. It was generally confined to the voluntary muscles, though it affected those of a mixed nature. It was sometimes detected in the muscular coat of the large intestines after death, which were found greatly contracted. In those cases the voluntary muscles were also affected. The muscles of the feet and toes, and the gastrocnemii, were the first in order of frequency, and next those of the hands and fingers. Then follow those of the arms and thighs, from which the spasms extend to the abdomen and more rarely to the chest. The diaphragm appears to be often affected with tonic spasm, from the manner in which the thoracic muscles become interested in respiration ; it is rarely, if ever, affected with the clonic variety, and

I have never witnessed an instance of hiccough of more than momentary duration. The contractions of the diaphragm are attended with distress, not with pain. The fingers and toes are distorted in every direction, and the limbs are sometimes thrown about with considerable violence. Then again this species of spasm suddenly subsides to the fixed contraction, or they exist in connexion, even affecting parts of the same muscle at the same moment. This coincidence I have particularly noticed in the gastrocnemii. The muscles of the abdomen are chiefly affected with the tonic spasm, by which the parieties are brought into permanent contact, and a singular hardness is the characteristic result. The body is rarely distorted in consequence, but occasionally the clonic spasm doubles the sufferer. The muscles of the eye, I believe, are never affected, at least with the clonic species, nor those of the eyelids, and very rarely the facial muscles. Those of the neck are more liable to convulsive twitchings. The thoracic and abdominal muscles were sometimes affected, in connexion, with tonic spasm. The bladder was evidently rigidly contracted in most subjects, which appeared to be rather the result of spasm, than its natural tendency to contract.

The greatest suffering attended the contractions of the simple voluntary muscles, especially the gastrocnemii, in which the pain was generally very great; evincing rather an exaltation than "diminution of nervous power." The affection of these muscles was very similar to that which occurs in hysteria;—rarely, if ever, like the movements in convulsions or epilepsy. There was no pain, but sometimes distress, attending the tonic spasm of the muscles of a compound nature. I am not informed whether there was any unusual suffering in the abdomen of the few in whom the large intestine was found contracted. Affections of the mind not infrequently excited the spasms.

This species of suffering would often produce great jactation, and the patient would even start from his bed in pursuit of relief.

Respiration was less laborious, than would be inferred from the embarrassed circulation in the lungs, the action of the heart, and the tonic spasm which frequently seized some of the muscles concerned in the process. Whenever the spasm attacked the abdominal muscles, it was obvious from the great labour of the others; and if the diaphragm became its subject, it was denoted by a great elevation of the chest, and the action of the alæ of the nose; and again the abdominal muscles especially participated. These varieties were frequently presented and were well marked. This function was generally slowly performed, and sometimes particularly so,—especially if there existed a determination to the brain. It was rarely attended by great distress, though it was frequently apparent that the patient was oppressed; a sensation next to thirst that created the greatest restlessness. The patient would lie for a short time in a state of apparent ease and abstraction,—then suddenly turn with a sigh, or a moan, throwing the covering from him, and was again at rest till the same process was repeated. The admission of fresh air relieved the jactation. The breath became cool at an uncertain period of the disease. It was almost a uniform and striking symptom.

The *Voice* was affected in most of the subjects; but its tone was variously modified. In some it was hollow and sepulchral,—in others it became flat, and oppressed, and again it was a whisper, or it was quite inaudible. At other times it was hoarse and rough, and in a few it continued natural. The patient generally spoke with effort, but was sometimes very colloquial. I have only remarked a disposition to converse in the intelligent and well informed; and in those instances, speaking did not appear to produce exhaustion; and was performed without difficulty when the voice was distinctly audible.

I have formerly remarked, that no indication could be derived from the state of the *tongue.* If premonitory diarrhœa had existed, in a majority of instances the tongue was coated in the centre of a dirty white, and clean at the tip and edges. In other cases this coating was more extended, superficial and lighter. Again, it was merely a sliminess which was easily detached, and soon returned. When presenting any of these conditions, the coating has been more dense and extensive during the existence of the antecedent diarrhœa,—the irruption of Cholera so modifying the action as to change the aspect of this condition of the organ; and it often continued decreasing during the progress of the disease. Under these circumstances the colour of the tongue was paler than natural, and the mouth was moist. In other instances a slight frosting appeared, like minute particles of sugar, through which a more florid and dryer tongue might be every where seen. Again there was only a roughness of the surface, resembling a coarse file, when the tongue was also florid and deficient in moisture. Or it was perfectly smooth and reddish and moist, presenting the aspect of beef; or it was clean and livid ; or it might be in every respect perfectly natural. I never saw it coated to any remarkable extent, and should consider a dense coating favourable. It approached its natural character most in those subjects who had been attacked without the common premonitions, and was always formidable. The mouth was almost constantly moist, though sometimes approaching a state of dryness. A dark sordes would often accumulate about the teeth just before death.

There was no symptom attending the tongue and mouth so uniform as the reduction of heat, and which I believe always occurred for some time before dissolution. I had always a thermometer with me, and applied it in a great number of cases, being careful that the patient had not drunk just previously. It was also an early symptom, and while pulsation existed in the radial artery it averaged about 92 degrees. Soon after the invasion it descended to 96, and progressively sunk to 86 or 88 degrees, when life became extinct. The lowest temperature I have observed was 81 degrees, in a dying subject. I have frequently found it 85, near the extinction of life, particularly in protracted cases. In one instance, in which the disease attacked without any premonitory symptoms, I found the temperature in six hours after at 84 degrees, and pulsation still existed; and in another, under exactly the same circumstances, it stood at 86.

At 94 degrees it became what is called a "choleric tongue," and was sensibly cold to the touch. At this temperature, also, the breath imparted a feeling of coldness, and injections came away with the same property. The accumulated heat at the epigastric, or the hypochondriac, region, did not affect the temperature of the mouth or the bowels. As it augmented in one, it diminished in a corresponding ratio in the other. These phenomena, and the coldness of the breath, contrasted surprisingly with the intensity of the thirst; but that I apprehend was owing to the morbid state of the stomach. I do not ascribe it to the accumulated heat around that organ, where I doubt not it existed within the cavity of the abdomen, in some instances at more than 112 degrees. I once examined a subject with Dr. Ludlow in 15 or 20 minutes after death, in which I have no doubt that temperature existed, as I think also, it must in the dying patient in whom I found the heat on the surface at 106 degrees. Neither do I believe the sense of heat or burning in the region of the stomach is connected with this exalted temperature. It is not a common complaint, and I have heard it equally made, when the surface of the epigastrium was cool. The patient was never disturbed by the introduction of the thermometer into his mouth, but would generally contribute some assistance.

The *Glandular Secretions* have been suspended in the same remarkable manner that has been every where observed; particularly of those organs which are most concerned in the functions of life. I have generally found scarcely a trace of bile in the liver, and the vesicle containing about the quantity which we find in subjects of other diseases. The same extinction of this function of the liver is equally denoted by the absence of bile in the discharges, with the few exceptions of which I have formerly spoken. When bile has been discharged during the progress of fatal collapse, it has exhibited a greenish appearance with a tinge of yellow, and has existed in abundance and without smell. There was no urine secreted in these cases. They were as malignant as the majority.

I have only seen two cases in which the urine was passed during a protracted collapse, and in which the patients were pulseless from the beginning. These cases occurred in the private practice of Dr. Lee, and are the two to which I have before adverted. It was a very rare attendant.

I now approach the condition of those organs which are especially connected with the great nervous system, and in which we should expect to find the most indisputable evidence of disease, did it consist in a "prostration of nervous energy." The great centre itself of sensibility gives no token that its energy is much impaired; but in presenting you with a faithful account of the varieties which occur, I must not neglect to say, that absolute insanity has in very rare instances taken place, when no trace of morbid action could be detected in the brain on dissection. At other times, though not often till the near approach of death, more or less coma has been present; but this has probably arisen, in part, from an impaired state of the senses, some of which are slightly affected in about half the cases. The hearing was more affected than any other sense. But I have never seen an instance of entire deafness. A singing and other noises were often heard, especially by the intemperate, though they rarely complained of the symptom. The sight seemed not to be affected in most instances, though sometimes quite impaired. A transient dimness of vision was not unusual, but subsided as suddenly as it supervened. The taste was evidently more obtuse; but patients generally answered that they tasted the medicine, though they rarely complained, excepting of its irritating properties. I know not how far the smell may have suffered, as the patient was too listless to attend to odours.

It would be but repetition to speak again of the remarkable ability with which locomotion was performed by many patients; but there were those who became powerless as in a state of intoxication.

I think there is sufficient evidence to show, that the varying affections of the mind and of the senses are not the result of any known morbid action in the brain and nervous system, and that these phenomena are generally less when they do exist, than would be inferred if explained on the principle of sympathy alone. This system, therefore, while it is less concerned than many other organs, in the functions of organic life, seems to exist in a state of particular independence.

The *Prognosis* is always unfavourable in every case of Asphyxiated Cholera, from its beginning. While the symptoms of oppression exist, there can be no other than the most fearful apprehension; and it is only as the vomiting and purging cease, as the pulse begins to swell, and the skin to recover its warmth, that we can encourage the hope of a favourable issue; and even then, the rising strength and hardness of the pulse, the accumulating fur on the tongue, the flushed face, and the injected eye, often denote that the danger is not over. Almost any change of colour in the discharges is auspicious, but particularly as it inclines to the darkest shade, when the odour, often strong and offensive, returns. Still, a putrid smell is bad. The quantity discharged is surprisingly great, contrasted with the little fluid that remains in the system. It assumes a variety of appearances in the progress of convalescence from black to green, and thence to yellow. It ultimately seems to consist of concentrated bile, at an early period producing heat and tenesmus; at a later, tormina or slight gripings. If the urine reappear, no symptom is more promising. When the attack is not preceded by diarrhœa, active treatment, early applied, is most successful in such cases, but delay is generally followed by a speedy death. The beef-like tongue is always fatal, and a clean tongue more unfavourable than a coated one, unless occurring without premonitory symptoms. Sordes on the teeth denotes the near approach of death. I have never known a recovery, where the temperature of the mouth has been actually below 94 degrees; and to this inquiry I have given particular attention. Profuse sweating forebodes death. The presence or absence of wrinkles on the skin affords no indication. Danger is great in proportion to the colour of the skin, and the inequality in the distribution of heat. A cold or a sour breath is fatal; great jactation is fatal; and so is coma, or any considerable affection of the functions of the sensorium. If complicated with organic disease of any important viscus, death is certain. The intemperate, I believe, die invariably. Sex has no influence. Old age sinks under the struggle, and children, if recovered from collapse, generally die of subsequent congestion of the brain. It is very difficult to arrive at any accurate estimate of the proportion of recoveries from the asphyxiated disease. The greatest obstacle arises from the little discrimination that has been made between premonitory diarrhœa, and the constitutional affection, the former being regarded as the first stage of the latter. It is manifest, that a great proportion of reported cures belong to the first class, both in Europe and America; and it is from the success which generally attends the treatment of the diarrhœa, that many worthy practitioners have declared that they are rarely disappointed in the results of their remedies. I think, however, I gave the most favourable construction to this subject in a former letter; and from subsequent observation, I may safely repeat the assurance, that the practitioner is eminently successful who saves one patient in six, if Asphyxiated Cholera be the disease. Truth obliges me to concede that such has been the melancholy result of my own efforts, and I feel an equal conviction that I do no injustice to others, when I impute to them as little success. It should be stated in behalf of the able and devoted gentlemen who have presided at the Hospitals, that they have constantly received the worst subjects of the disease, and often when in a dying condition. Still, I do not believe there has been more honourable experience in private practice.

It is stated on high authority, that the ratio of deaths in Asia was only 1 in 7 or 8, while in Europe it was 1 in 2 in the *asphyxiated stage.* I have no doubt of the far greater success of the Anglo-Asiatics from the absence of antecedent diarrhœa, which enabled them to adopt blood-letting with so much advantage; but I must doubt as to the proportion of recoveries which are said to have been effected, whether in Asia or Europe. Magendie has said that "Cholera begins where other diseases end," and we find all authors speaking in the language of despair of the asphyxiated disease; save only the projector of some new remedy. Dr. Holmes, of Montreal, writes me that, "in the fully developed disease, scarcely any treatment is of avail; and this opinion coincides with that of the practitioners with whom I have conversed."

Convalescence was slow, and often embarrassed by the developement of topical inflammation, or congestion, observing the following order in respect to frequency; the brain, the liver, the small intestines, the stomach, constituting what has been rather inconsiderately called *typhus fever,*—and that, too, without much reference to the particular organ which might happen to become most involved in the stage of reaction. This second period of the paroxysm was almost invariably attended by symptoms denoting congestion of the liver, and when they did not extend to the other organs, especially the brain, the general phenomena were those of mild febrile reaction, bearing no correspondence with the violent irruption of the cold stage. The constitutional symptoms were more modified by congestion of the brain, when the pulse became full and hard, the skin dry and hot, the tongue dry and furred over, the face flushed and the eye injected, generally stupor, and sometimes delirium, but very rarely restlessness. It was an obstinate and fatal condition of disease. If coma were not soon relieved by blood-letting, the patient gradually sank; but partial relief encouraged its repetition, and frequently with success. Sordes on the lips and teeth was a fatal symptom; so was a small tremulous pulse, protracted respiration, and insensibility of the eye.

Perhaps it may not be unacceptable to advert, in this place, to the mode of practice most successful in the more auspicious terminations of the cold stage, and what are denominated its *consecutive diseases.* When the second period of the paroxysm ensued without any particular local disturbances, it was constantly necessary to act with a reference to many possible consequences,—particularly to the developement of cerebral congestion. But so far it was rather a system of watching than any active treatment. From the functional derangement of the chylopoetic viscera, it was especially necessary to exhibit cathartics of calomel, and castor oil, and to subject the patient to a very *rigid diet,* and the most temperate and salubrious habits. I do not think that any benefit arose from the use of means internally, that were addressed specifically to any one organ,—as nitric ether to promote the secretion of urine, &c. Some very intelligent practitioners have employed Dover's powder, either uncombined, or associated with calomel; but I have thought its tendency was to produce congestion in the abdominal viscera, and subsequently of the brain. If the liver, as not infrequently happened, became the seat of greater disease, either local or general blood-letting was necessary, and a large blister over the region of that organ. The general principles of treatment were continued.

The brain being more constantly the organ to which the local determinations tended, it was often very difficult to conduct the convalescence of the patient safely along. This complication of the disease was sometimes insidious in its approach, but its full developement was well marked, and generally such as I have described. It pursued the patient with great tenacity, and would often recur with violence when partially subdued. Hence it was necessary in all cases, without much reference to contingent circumstances, to pursue an active system of depletion. General blood-letting was indispensable, and it was often necessary to repeat the operation several times, and that in rapid succession. When a sufficient impression was made by this means, leeching and cupping came in with great advantage, and blisters to the neck and head,—the hair having been removed as soon as the brain was obviously diseased. I found ice, also, a useful topical application. I exhibited calomel freely, either in large or small doses, according to particular indications, some of which were especially derived from the state of the liver and bowels, the former being always congested in these cerebral affections. Hence, also, blisters over the abdomen were powerful auxiliaries. Every substance in which opium was an ingredient aggravated the whole train of symptoms. The warm bath, not exceeding 98 degrees, was useful after the vascular action was subdued. Antimonials and ipecacuanha were excellent alteratives in small doses, frequently repeated. Rest and darkness promoted the good effect of every other remedy.

Relapses of the asphyxiated form of the disease were less common than has been generally represented; and I am happy to bear my testimony to the correctness of the opinion expressed by the medical council, that "a second attack was not to be expected." This opinion has been censured; and doubtless, it was not sufficiently qualified. Cholera Asphyxia has been known to affect the same subject twice in quick succession, but this is a very rare occurrence. What are denominated relapses may be thus explained. A patient is treated for the premonitory diarrhœa, or for some other condition of local disease, which may have been supposed to announce the existence of Cholera, in what has been called its forming stage; and he is soon relieved by his medical attendant. Subsequent exposures develope the constitutional disease, and this man is said to have a second attack of Cholera. Or another patient is restored from the collapsed disease, and the same exposure subjects him to the most formidable congestions of the abdominal organs, or of the brain, and this subject perhaps is said to have died of a second attack of Cholera, or of typhus fever. It is even a matter of pride with the unreflecting, to boast of their recovery from a second attack; which is found on inquiry to have consisted of some looseness of the bowels, or a very natural ejection of some irritating food from the stomach. In this manner has arisen the common impression, that second attacks of Cholera are not infrequent.

It seems almost superfluous to advert to the *Diagnosis* of this disease. If it possess no unvarying characteristic, it may be always distinguished by the uniform presence of some combination of symptoms that is strictly pathognomonic. It is constantly confounded with every species of diarrhœa, from the assumption that a common antecedent affection of the bowels is an integral part of the subsequent collapse; but none are so blind as not to distinguish the period of constitutional invasion. Even the active operation of a cathartic has subjected many a worthy citizen to the distinction of being a recovered subject of Cholera. Still it is better to inflict on the hapless "cholerine" all the pains and penalties that can arise from cataplasms to burning alcohol, than permit his diarrhœa to persevere. When this disturbance of the bowels has proceeded from atmospheric influence, I think the patient is more regardless of his health, than when it arises from other causes.

Ordinary cholera morbus is less frequently mistaken for Asphyxia, than the latter is confounded with "premonitory diarrhœa." Their specific difference is easily distinguished.

The spasms in hysteria are more like the clonic movements of the muscles in Cholera than those of any other disease; and the coincident sinking of the pulse, which often occurs in either affection from the influence of the spasm, renders a mistake possible. The milder disease always excites great consternation during the prevalence of Cholera.

I have seen the operation of a drastic cathartic result immediately in Asphyxiated Cholera. In such instances, the patient is ignorant of his situation, and awaits the further operation of his ill-directed dose, until beyond all antidote.

Post mortem appearances. You will have seen from the cases which I have heretofore transmitted to you, that there exists a very great variety in regard to the appearances which are observed on dissection. More recent observation justifies the statement which I made you on this subject; but as that was very general, I shall pursue the original plan of this letter of representing the appearances more in detail. I may now premise, that I never detected any other remarkable degeneration arising from this disease, than what I have noticed as existing in the alimentary canal, and the flabbiness of the heart.

External appearances. There was entirely less tendency to decomposition in the subjects of Cholera in this city, than in Asia; and thus far the coincidence is more with the European disease,—a circumstance which may lead us to ascribe something to the influence of tropical heat. I think, too, that the evidences of such change as did occur, were as strongly denoted immediately, as a few hours after death; but rather consisted of partial disorganization arising from morbid action, than from the operation of chemical laws. Even the flatus in the bowels was not particularly offensive, till six or more hours after death. It should be stated, however, that our season has been unusually cool, and probably, therefore, the phenomena of early decomposition, which were noticed in the East, were here delayed by the operation of that cause.

The body was generally rigid, and always so if spasms were among the last symptoms,—the extremities, particularly the arms, being sometimes in a contorted position. The general surface was often warmer for a few hours after death, than when the patient expired. The skin, on the superior part of the body, usually retained more or less of the colour, which had constituted one of the symptoms. Sometimes, however, it would disappear soon after death, and in an hour or so return in varying shades. It was commonly most remarkable on the face, scrotum, and hands. At other times it would soon vanish not to return; or if the skin had not been discoloured during life, it would generally remain so after death,—or in other instances a general lividness, or patches of blue or brown, would ensue. The corrugation of the fingers remained, though less distinctly. The square form, which I have noticed, was partially lost in a more withered appearance. There was no remarkable shrinking of other parts of the extremities. The parietes of the abdomen were sometimes contracted, and sometimes inflated;—but more generally natural. The features were more shrunken than in life.

The *Eye* was often lustrous soon after death, and commonly turned upwards. It very rarely exhibited the *film* described by Eastern writers.

The *Teeth* and *Lips* were frequently covered with sordes, and the *Tongue* often became opalescent after death. At other times they were natural.

The *Muscles* were generally firm, and more florid, as soon as divided, than any other part.

Abdomen. The stomach and intestines were often inflated immediately after death. Again there was nothing remarkable, in this respect, at many subsequent hours, and they were seldom in a collapsed state.

Stomach. The exterior of the organ was generally natural. I never saw it contracted, even at its orifices. The serous tissue sometimes presented a blush of redness, or it was occasionally more distinct in patches. The larger vessels were not often injected. The mucous coat was frequently florid at one or both extremities, or the redness occurred in spots of various shades from a crimson to a deep purple or black. These appearances occurred chiefly in the intemperate, and were rare in those of good habits. Sometimes the membrane was unnaturally white, but not blanched as in the large intestines. It was frequently corrugated, but rarely if ever thickened or softened in its texture. The mucus was seldom natural, and not often superabundant. In a pathological sense, its appearance was entirely the most important phenomenon attending the organ. It was about the consistence of cream, of a grayish white, and more analogous to that substance than to ordinary mucus. It generally adhered to the surface, or when more abundant, was intermixed with the other contents of the stomach. I never observed an appearance of mucous follicles. Among the contents of the organ were frequently found some of the solid ingesta, and there was generally more or less of a fluid similar to that which had been ejected. Bile was sometimes observed, but it was a post mortem occurrence, where it had not been vomited. I have made no experiments to ascertain the presence of an acid here, or in the intestines.

Small Intestines. The organs generally denoted some morbid vascular action. The usual colour presented on opening the abdomen was pinkish, a lake red, and sometimes florid. This colour existed most conspicuously in the inferior portion, and gradually diminished as we ascended to the duodenum. It terminated abruptly at both extremities, in most instances. If it resided in the serous membrane, it appeared not to exist in the minute ramifications of the vessels, and seemed to be restricted to the venous system. But in a great majority of cases the serous tissue was natural, and the discolouration was in the membranes beneath. I never saw the muscular coat contracted. The mucous tissue, and the connecting cellular substance, embraced, in most instances, the vascular fulness, which appeared to be generally limited to the veins. Some times, however, the injection was very minute, and a florid redness extended over the whole surface of the membrane, here and there increasing in brightness. It often resided chiefly in the cellular tissue beneath.

Again no trace of colour could be detected beyond the serous tissue, or more rarely it was absent in both. The degrees of colour, evidently, often depended on the duration of disease, the extent of the evacuation, and the time of examination; but it was much less connected with the habits of the patient than similar appearances in the stomach. I never saw the small intestines blanched. The villous coat was frequently softened in texture, which sometimes extended also to the serous, and the former occurred when there was no fulness of its vessels. In a great proportion of cases, however, there was no such change. The membrane was rarely thickened. The valvulæ conniventes were sometimes more than naturally prominent, and gave to the membrane an appearance of intumescence. The coats were often easily detached from each other, and constituted a pathologiac appearance, which might happen when firm in their texture. The glands of Brunner and of Peyer, especially the former, were often observed, and they sometimes studded the membrane. They abounded most where vascular action was most obvious, and were generally florid. The mucus was more abundant than in the stomach, and adhered to its membrane ; or it sometimes existed in a very preternatural quantity, and was then intermixed with the other contents. It was usually of the consistence of cream, and still more like that substance than was the mucus of the stomach.

The contents of these organs varied in appearance, and were not always similar to the dejections. They commonly resembled the notorious " rice water," or oat meal gruel. We did not often find the thick opaque substance filling the canal as described by Christie and some others. Sometimes there appeared to be a mixture of the creamy mucus with the more fluid contents, and the flocculi, noticed during life, were often abundant. I have seen the whole tract of the small intestines filled with a deep yellow fluid, when none of it existed in the large intestines. It was not unusual to observe traces of bile in the duodenum, which had evidently exuded or had been pressed into that division of the intestines after death. The intestines had a doughy feeling.

Large Intestines. In these organs we had a far greater variety in appearances, from a pinkish hue, to a whiteness which arises from long maceration. These appearances were greatly influenced by the extent of the evacuations, and the duration of disease. There were observed the various gradations between those extremes, but the prevailing was a blanched appearance. At other times the whole extent of the organ was natural as in health, and some slight change in the mucus was the only circumstance denoting morbid action. When any redness existed, it was generally in the cellular tissue, but red patches were occasionally noticed on the villous coat. The cæcum was not particularly the seat of appearances,that did not generally exist in the other divisions. The mucus was least abundant when the membrane was most blanched, and it generally partook of the character of that which existed in the smaller organs. The follicles were not often enlarged, and when so, I commonly thought that it was owing to chronic diarrhœa. The texture of the membranes was rarely softened. Contractions were occasionally observed, affecting the entire tract of these organs, or only particular portions, especially the arch of the colon. They were always very remarkable in degree, sometimes scarcely admitting the finger. Intussusceptions were rarely observed. The contents were generally watery, of a light colour, sometimes brown ; and again of a consistence like cream.

There often existed on the serous membrane of the intestines a transparent, glutinous fluid.

There was generally a characteristic appearance of the abdominal viscera, in situ, in Cholera subjects ; which chiefly consisted in the contrast of colours that resided in the two great divisions of the intestines.

Liver. No organ was less uniform in its aspect. It sometimes exhibited the appearance of a mass of clotted blood, and again it was as natural as in health. Between these extremes various gradations occurred, and it was not unusual for one lobe to appear more discoloured than another. Its serous coat exhibited no mark of disease. Organic lesions were seen in the intemperate, but evidently of antecedent origin. They did not appear to influence the determination of blood to this organ. The most remarkable and uniform appearance, on making incisions into its substance, was the absence of bile,—frequently scarcely a trace being observed. It was sometimes gorged with blood, but more commonly contained its natural quantity, or sometimes appeared exhausted of that fluid. The vesicle always contained a considerable quantity of bile, generally about an ounce and a half or two ounces, sometimes not half that quantity, but often was rather distended. The colour varied much, the most prevailing being a greenish yellow,—the extremes being black and yellow. Its consistence was generally natural. The vesicle showed no mark of disease in its mucous membrane. Gall stones were not common, but were sometimes observed. It rarely happened that the bile would not flow freely into the duodenum on compressing the vesicle, or that a probe might not be passed through the ducts.

Pancreas natural.

Spleen generally very natural, but sometimes gorged with blood.

Kidneys natural, their papillæ often affording small quantities of a fluid like semi-purulent matter. They sometimes exhibited a plethoric appearance.

Bladder, generally empty, and very much contracted. In a few instances it was found to contain a natural urine, to the amount of six or eight ounces. Sometimes a small quantity of a semi-purulent-like fluid was found in its cavity. Its lining membrane was generally natural, but sometimes injected.

Epiploon generally exhibited a fulness of its veins, but no minute injection.

Mesocolon and *Mesentery* had their veins more or less injected in most subjects, depending in some degree on the position of the body subsequent to death, and the time of dissection; as there was reason to think that the blood had sometimes returned to the larger branches of the vena portæ.

Thorax. There was nothing uniform in the appearances in this cavity. Sometimes the lungs were found occupying the full extent of it, or half collapsed, or compressed into a withered appearance. When their volume was thus remarkably diminished, it would have seemed almost the indispensable result of some mechanical cause, had it not been otherwise determined by experiments in the East. It was a pathologiac appearance which involves an interesting inquiry. When their structure was affected, it was from antecedent disease. In respect to vascular fulness, appearances were similar and equally as various as in the liver, being sometimes surcharged with blood, and then again through all the intermediate gradations to a state of almost entire exhaustion. A plethoric state was sometimes attended with an abundant production of a fluid within the bronchiæ.

The *Heart* and *Blood-vessels.* The *heart* presented no other unusual change than a frequent softening of its texture, by which it acquired a flabby feeling and appearance. Dark patches were sometimes seen on its surface. These were pathologiac conditions evincing something more than impaired nervous energy. The organ was rarely injected, even when the purple patches appeared. Its cavities were often full; or were sometimes entirely empty; or the right cavities were filled and the left empty; or more commonly, a smaller quantity was found in both ventricles. The blood was equally dark in all the cavities. If the ventricles were empty, it did not follow, that the blood was found in the venæ cavæ, though sometimes considerably accumulated here; but at others, they were empty, and it was then obvious, that the blood had been arrested in one or more of the great viscera. When there was blood in the left cavities of the heart, it did not extend far into the blood vessels. Even the aorta contained but small quantities, and its subdivisions were empty,—generally not even stained with blood.

The *Pericardium* was natural.

There was no effusion in the cavity of the thorax.

I have already explained the appearances of the blood, and shall therefore avoid the repetition.

Brain. In about three fourths of the cases, this organ has presented a vascular fulness of its membranes. The great sinuses and the veins of the dura mater were often gorged, and the pia mater was so minutely injected, as to have exhibited, in some places, an appearance of extravasation, which indeed might have existed. Again there was observed a fulness of the vessels in one membrane and not in the other, or only partial discolourations; or, in many instances they were perfectly natural, or even deficient in blood. It was not unusual to observe a fluid beneath the arachnoid, sometimes rather abundant, but generally small in quantity. The brain was sometimes moist, and in respect to consistence, about the same varieties occurred as in subjects of other diseases. On dividing its substance, blood would sometimes appear in small dots, and more abundantly when expressed. But I could rarely perceive that this was a pathologiac appearance. I never remarked any unusual appearance in the complex parts of the organ. The ventricles frequently contained a preternatural quantity of fluid, and were often dry. The plexus was generally natural, but sometimes full. I never discovered any thing worth recording in the cerebellum. The pons was natural, and the basilary artery contained black blood. The nerves were natural. As far as my knowledge extends, the spinal marrow exhibited no other evidence of disease than a fulness of the exterior vessels. The ganglia were not found to present more than, occasionally, a slight redness.

On looking over the preceding remarks, I find that repetitions often occur; but they were unavoidable in a detail of symptoms, succeeding to a general description of the disease.

Very respectfully, &c.

PATHOLOGIAC VARIETIES OF CHOLERA.

LETTER VII.

New-York, *July 3d*, 1832.

Dear Sir,

The reports respecting the existence of Cholera in this city are so contradictory, that you may be desirous of knowing the absolute truth; but I am chiefly induced to make you this communication for the purpose of submitting to you the morbid appearances on dissecting two individuals who were supposed to have died of this disease, and the notes of which I took during the examination, which was conducted by Drs. Carroll and Ludlow, and myself.

The subject of the first case is the man who lived at the corner of Reed and Greenwich streets. I did not see him till about twenty minutes after death, and have derived my knowledge of his symptoms from his wife and his physician.

He was forty years of age, very robust and corpulent—pursuing the trade of piano-forte making—of intemperate habits, and had drank rather excessively on Sunday, the 1st instant, though was sober enough to accompany his wife to church in the evening. He had complained of a sense of tightness about the region of the stomach, for four or five days previously, but at no time of pain in his head, before or during the disease. His appetite remained good till the noon of that day, when he ate moderately of fried fish and potatoes. Soon after his return from church, he retired to bed, remarking to his wife that he had an unusual commotion in his bowels, which occasionally interrupted his sleep till twelve o'clock, when he first suffered the attack of vomiting and purging, attended by great distress in the abdomen.

The house was immediately deserted by the other occupants, and no messenger could be obtained by his wife, to procure a physician, till five o'clock in the morning, at which time the patient was visited by Dr. Cameron. The doctor informs me that he found him violently affected with vomiting, purging, and most convulsive spasms; the features sunken and the eyes staring; the pulse insensible at the wrist, and the surface cold, and covered with a clammy sweat; the countenance black and terrific; tongue of a dark purple during the spasms, becoming opalescent as the spasmodic action abated; the fluid rejected was watery, consisting probably of the liquids he was permitted to drink; his dejections resembled rice water, of the consistence of cream. The faculties of his mind were unimpaired to the last, and during the short intervals of ease, his muscular strength was not sensibly diminished. He passed no water after the arrival of the physician.

Small quantities of laudanum were administered, and the usual external applications made, with the effect of diminishing the symptoms generally for two or three hours, at which time reaction began to take place, and at ten o'clock he was bled to the extent of ten or twelve ounces. The symptoms, however, had been again increasing, and continued to become more violent till the time of his death, which took place about eleven o'clock, the patient having suffered about eleven hours.

The lividness disappeared from his skin about half an hour before death, and returned, particularly to the face, about an hour after death.

I first saw him about twenty or thirty minutes after he had expired. The features had recovered from their contraction, and with the exception of the eye, presented no unusual aspect. But the eye was remarkably expressive of that influence which the disease has been supposed to exert on the nervous system; and its wild, glaring stare was startling to the spectator. It was looking forward, and was much exposed by the contraction of the eyelids.

The muscles of the body were rather rigid, and the surface very cold. Spasmodic contractions of the lower extremities were still taking place; and

I am informed by Dr. Cameron, that the legs continued to move occasionally for an hour and a half after his death. The tongue and inside of the lips exhibited a dead white, with a slight shade of blue. This colour was independent of the coating, which did not exist to any remarkable extent. There were several livid patches on the thorax and shoulders.

The dissection was commenced in about an hour and three quarters after death. At this time the face had again become very livid, and the skin was discoloured extensively in other parts. The eye retained its remarkable wildness, much of which was remaining four hours afterwards, when we finished the dissection. The muscles still slightly rigid, but no spasmodic motion could now be observed. The incision penetrated about three fourths of an inch of cutaneous fat, and I was surprised to find the muscles so florid in a case of so much obvious congestion.

On opening the abdomen, the stomach and large intestines appeared very much inflated ; the small intestines only partially so. The stomach and small intestines were florid, denoting great vascularity of the internal coat. Large intestines natural. Mesentery and omentum loaded with fat. No turgescence of their veins, or of the veins of the diaphragm. No fluid in the cavity.

The stomach contained a pint of a fluid resembling a weak infusion of coffee. A bright redness existed in the upper portion of the stomach, occupying about one third of the organ, and terminating abruptly near the cardiac orifice. All the inferior portion was slightly livid, and, at the pyloric orifice, a livid redness began abruptly, and extended through the whole tract of the small intestines.

In the large intestines, the mucous membrane was natural. A large quantity of a fluid, resembling oatmeal gruel, and of a shade between that and rice water, existed throughout the intestinal canal. There was rather more than the usual quantity of mucus, resembling cream, though not so white.

Liver externally natural—perhaps rather lighter than usual; on making incisions, the black blood flowed more freely than in a healthy state of this organ. The gall bladder contained about an ounce of black bile. The ducts perfectly natural, the bile flowing freely through them. Spleen and pancreas natural. Bladder empty, but not particularly contracted. The veins of the right auricle of the heart rather fuller than usual. No turgescence of the coronary veins. Lungs blue, and turgid with black blood to a very remarkable degree. Cavities of the heart and venæ cavæ entirely empty. No fluid in the thorax. Brain engorged with black blood, and the ventricles contained four or five ounces of serum. We were not permitted to examine the ganglia and spinal marrow.

Case 2. We immediately proceeded to examine the case of Betsey, the black girl, who resided as cook at Mr. Thomas', No.185 Reed-street, about twenty rods from the house of Mr. G., and where she had lived for several months. Betsey was very temperate and regular in her habits, but a large feeder—of slender frame, but capable of her duties, and generally enjoyed good health; about twenty years of age. She attended church in the afternoon of Sunday, the 1st instant, and made no complaint till six o'clock of that evening, when she felt some sickness and pain at her stomach, and went to bed. At two o'clock, A. M., the family were awakened by her groans. The morning after the examination, I received from her attendants the following account of her symptoms:—She was found stiff with the cramp, and severely affected with vomiting and purging; complained of great distress over the whole body; eyes staring and sunken, and the features contracted. Fifteen drops of laudanum and a little camphor were given, which removed the vomiting in a few minutes. The same dose was repeated in an hour after. The purging, however, continued, and the spasms constantly recurred, attended by great suffering. Profuse sweating took place towards morning, when the surface became rather warmer. At eleven o'clock, she began to sleep, and was then left by her attendants till four o'clock, at which time she was found dying.

The fluid vomited and dejected is said to have resembled rice water. She had no physician. In the progress of the disease, a dose of salts was given, and occasionally wine and water.

The dissection was commenced at fifteen or twenty minutes after death. Every muscle was violently contracted, and remained so, without any motion, however, for an hour and a half, when the examination was finished. Skin livid, and petechiæ numerously distributed, appearing with great distinctness on the schlerotic coat of the eye. Features rather sunken; expression of the eye remarkably wild and glaring, corresponding exactly with this appearance in the other case. The eyes were also very much exposed, and turned upwards.

The stomach and large intestines externally natural, and free from flatus. Small intestines livid. Veins of the omentum and mesentery more than usually full. Mucous coat of the stomach slightly discoloured; scarcely a morbid appearance. This organ contained a small quantity of a fluid resembling rice water. The lividness of the small intestines existed in the serous membrane, the mucous coat being natural. These organs contained a considerable quantity of a yellow fluid. The large intestines were filled with a fluid exactly resembling rice water, with flocculi like "half-dissolved flakes of snow." Mucous membrane natural. The mucus, which resembled a creamy substance, did not exceed the natural quantity.

Liver very purple—the left lobe in a plethoric state, and the right lobe very turgid, with black blood. The gall bladder contained about an ounce of yellow bile. The ducts perfectly natural. Spleen and pancreas natural. Urinary bladder contracted to a small, hard knot, evidently owing to the spasm. No fluid in the cavity of the abdomen. The lungs were found in a perfectly natural state. The heart was engorged with black blood, as were also the venæ cavæ. No fluid in the thorax. We were too much fatigued to enter on the examination of the brain, &c.

July 4.—Cases of this disease are now constantly occurring, and it attacks all ages from infancy. The symptoms are uniformly such as I have described. Dr. Bliss informed me to-day that he witnessed the dissection of a child, in which the morbid appearances were exactly such as I have described in Betsey's case. The invasion is very sudden, and without any premonitory symptoms. I believe some have recovered. They generally die in about twelve hours from the attack. It has now appeared in almost every part of the city, and no one thinks of tracing it to contagion. The vomiting is easily allayed by small doses of laudanum. The spasms are obstinate. The collapse is immediate, and no reaction takes place. In short, it is death at the very invasion. The probable number of deaths to this time, six P. M., is from thirty to forty.

Very respectfully, &c.

LETTER VIII.

New-York, *August* 15*th*, 1832.

Dear Sir,

I have just attended an examination of one of the most malignant cases of Cholera Asphyxia that I have yet witnessed; and perhaps you may be interested with an account of the morbid appearances.

The subject was a very robust sailor, about forty years of age, and of intemperate habits. He was attacked with vomiting and purging on the evening of the 12th inst., and was found in a shed on the following morning, from whence he was taken to the Park Hospital, where he died in a very short time. The purging was now slight, but the vomiting and spasms were excessively severe. He had, also, the other characteristic symptoms of the disease in a prominent degree. Almost the entire surface of his body was nearly black.

The examination was commenced about ten hours after death. The surface of the body still retained its blackish hue, and the muscles, as is common, were very rigid. As their substance was divided, we observed the redness which they usually present in this disease, which contrasts very strongly with the blood in the larger arteries. The stomach was unusually capacious and rather florid. The small intestines were of a dull pinkish hue, which became less obvious in the larger. These organs were all inflated, but nowhere contracted. The veins of the omentum and mesentery were fuller than in the natural subject, and there was an obvious determination of blood to these viscera. The stomach contained about three gills of a fluid similar to the contents of the gall bladder, which had evidently exuded from the liver since the death of the subject, as the fluid vomited exhibited the appearance of rice water to the last moment of life. There were florid patches distributed on the mucous membrane, and its veins were fuller than natural. But I have generally seen these appearances only in the intemperate. I examined the texture of this membrane through the alimentary canal, and we were satisfied that it was not softened, or in any respect disorganized, the reverse of which, however, I have observed in cases of apparently much less malignancy. The pyloric orifice was in a healthy state, as was also the cardiac. The mucus was natural in appearance and quantity, and equally so in the intestines. The mucous coat of the small intestines was fully injected, and the blood florid, appearing more like inflammation than congestion ; though certainly not active inflammation. The large intestines were hardly in a pathologiac state, and manifested no particular vascularity at the caput coli. These organs contained a light-coloured fluid. The liver was unusually large, of a hardened texture, very natural in colour, and containing rather less than its natural quantity of blood. The gall bladder was full of a greenish yellow fluid, and its ducts entirely pervious. The kidneys, spleen, and pancreas, natural. The bladder moderately contracted, and containing more than half an ounce of a fluid resembling thin pus. The lining membrane was morbidly vascular, of a bluish appearance, but we could not detect any evidence of gonorrhœa. The veins of the diaphragm were not full. The lungs filled the cavity of the thorax. (We frequently find them very much contracted and empty.) They were black and greatly engorged with blood. There exuded from the incisions a large quantity of a muco-serous fluid. Their structure was healthy. The heart was natural, and contained in both ventricles black blood. The large arteries remote from the heart, as is common in our examinations, were entirely empty, not even stained with blood. There was no effusion in the thorax, or other cavities. The brain, quite unexpectedly to us, exhibited the most perfect appearance of health. Its blood was not even so much discoloured as in other organs, and there was no more than its natural proportion. There was no effusion under the membranes, and the ventricles were only moist.

Very respectfully, &c.

LETTER IX.

New-York, *August* 30*th*, 1832.

Dear Sir,

I attended this morning—at the Crosby-street Hospital, through the politeness of Dr. Rhinelander, who has the charge of that institution, and where I have before witnessed very interesting dissections—the post-mortem examination of two subjects who died of Cholera, and whose cases I communicate to you, for the purpose of announcing, if I am not mistaken, the discovery at that hospital of an oil existing in a free state in the blood. I have not seen this circumstance noticed by any writer ; or if so, I have forgotten the fact. Dr. Gale, the chemist employed by Dr. Rhinelander, is the gentleman who first observed this phenomenon, and who has witnessed it in many instances. He has obligingly furnished me with his notes on the subject, which I shall append to the cases. Perhaps, too, you may derive some interest from the novelty of one of the patients having been transfused.

A. B., an athletic negro, aged about forty years, employed as a sailor, and of intemperate habits, was admitted, on the morning of the 29th, with the usual phenomena of Malignant Cholera. Vomiting and purging had occurred to a large extent. He died at three o'clock in the afternoon, and the dissection was commenced about eighteen hours after death. There was nothing remarkable in the external appearance of the body. The muscular substance presented the usual florid appearance. No sensible decomposition had taken place. The veins of the epiploon and mesentery more than naturally full. Small intestines of a pinkish colour ; large intestines nearly natural—not blanched ; caput coli natural. These organs had a glutinous fluid on their exterior, which is often observed. The mucous tissue of the small intestines rather florid, but not minutely injected—the blood evidently existing in the veins ; its texture firm, but not very grumous ; the quantity of mucus natural, but creamy. The contents of the whole tract consisted of a large quantity of a fluid resembling oatmeal gruel. There was nothing unusual appertaining to the large intestines. The liver was very natural, and the gall bladder about half full of a light-coloured bile, which escaped, on pressure, into the duodenum. There were also found three gall stones, of the size of a pea, in the duct. Bladder contracted, and containing about two drachms of a turbid yellowish fluid ; quite vascular at its posterior part. Kidneys very large, but natural; a semi-purulent fluid oozed from the papillæ. Spleen flabby, and full of blood. Pancreas natural. The anterior portion of the lungs contained more than the natural proportion of blood, and the posterior still more—making a proper deduction for the position of the subject. The lungs otherwise healthy. The heart in all its cavities, and the large vessels, contained a considerable quantity of black blood. The substance of the heart was soft and flabby, otherwise natural. Numerous masses of firm coagulable lymph were found in this organ, and in the aorta at its origin—a circumstance which frequently occurs, even when dissections are made soon after death.

No fluid in any of the cavities.

The brain was perfectly natural—no fulness of its veins—perhaps rather less serum than should have existed. The arteries, as usual in choleric subjects, contained black blood. The par vagum and other nerves very natural.

The oil, to which I have adverted, was remarkably abundant in this subject. It covered the blood from whatever part abstracted, and floated in small and large globules on the surface of about one ounce, after standing an hour in a vessel, into which it had been received from the heart. There were two globules of the size of a common pea; the others were very numerous, but no larger than a grain of mustard. It was found in the large and small vessels—it was very distinct in the brain—it was as obvious in the kidneys, and in every part particularly examined. It resembles olive oil in appearance and consistence.

Case 2.—The subject of this case was a white man, aged thirty-five years, a labourer, and intemperate. He was admitted in the forenoon of the 29th inst., and died at eight o'clock in the evening. His symptoms were very malignant, and presented a good specimen of the Asiatic Cholera. He was transfused, five hours before death, with forty ounces of the common saline preparation. Dissection began fifteen hours after death. No particular evidence of decomposition. Body still quite livid. Muscles more than usually florid. Large veins of the abdominal viscera rather fuller than natural. The omentum more than usually vascular and florid. The stomach externally and internally natural—no fulness of its vessels, and no softening of the mucous tissue. Small intestines of a light blush of redness, but scarcely pathologiac; inner membrane rather more than naturally vascular, and the blood existing in the extreme vessels; its texture firm, and distributed on its surface were numerous enlarged follicles. The mucus here, as in the continuous organs, natural in quantity, and of a creamy consistence and appearance. Large intestines perfectly natural. These organs were much inflated, and contained a large quantity of a light-coloured fluid, intermixed with a pultaceous substance. Their surface was covered with a transparent glutinous fluid. In the stomach were found several large pieces of pickled cucumber, which had been swallowed without mastication. In the large intestines, as also in the other subject, the mucous tissue was easily detached, by pulling, from the serous. Not so in the small intestines. I have often remarked this circumstance in other subjects, and have found it existing in the small intestines. The liver was natural, and the gall bladder distended with black bile, which was easily expressed into the duodenum. The kidneys natural, and the same muco-purulent fluid as in the other case exuded from the papillæ in a small quantity. Spleen and pancreas natural. The bladder was not contracted, and it contained half a pint of a light straw coloured urine. This is the first instance in which I have found more than half an ounce of that fluid. The lungs, posteriorly, were gorged with black blood ; their structure healthy. The cavities of the heart and large vessels contained rather a large quantity of *black* blood, in which were discovered large concrete masses of coagulable lymph—some of them in the aorta, and some in the pulmonary artery. The substance of the heart was soft and flabby, and a few purple patches were observed on its surface—an appearance which is not common with us. The brain was in all respects natural ; its consistence usual ; the blood-vessels not half filled ; but there was more than half an ounce of serum in the ventricles.

In this subject the free oil was seen floating on the blood which was taken from the heart, from the kidneys, the brain, &c. ; but in less remarkable quantities than in the former case. I could perceive no difference in the colour of the blood of this injected subject, from what occurs in all cholerics, excepting in the minute vessels of the membraceous organs, and in the brain, where it was more florid. I think, also, the fluidity was greater, although the quantity transfused was not large.

In an unfinished letter to you, I have adverted to the dissection of another transfused subject, in which the appearances are nearly coincident with this case.

The following is the statement referred to from Dr. Gale:—

Dr. M. Paine,

DEAR SIR,—Your request concerning the oily matter which I have noticed in the blood, I am happy to gratify, as far as I am able. The oily matter referred to above, is not that described by Lecanu, as discovered by Schwilgué. The former is obtained by solution of the solids contained in the blood, in alcohol; the latter is found floating on the surface of the blood when taken from the body. The proportions vary with the stage of the disease, increasing as it is more advanced. Indeed, I have scarcely found any appreciable amount in patients that were not in a stage of collapse. It is greater after death than the usual increased proportion would furnish; or in other words, the proportion is a constantly increasing one. The case you saw here last, I think, is a fair sample exhibited by post mortem examinations. The portion of the oily matter in eight cases has been from one half to two per cent., which is the greatest proportion I have seen in any case.

The matter above mentioned, I believe, has not been described by any writer on the subject; and though I have by no means found it in all the patients examined, and from whom I have taken cholera blood, yet in every case, except one, of post mortem examination, I have perceived the oily matter, sooner or later, floating on the surface of the blood.

I have not yet ascertained whether it be a peculiar principle, or whether it be only the same as that described by Lecanu. I shall examine this matter as soon as I find leisure.

Respectfully yours, L. D. Gale.

The case alluded to as "a fair sample," is that of the sailor. The blood was rather more than usually abundant in the heart and large vessels, of diminished fluidity, and very dark in the arterial system.

Very respectfully, &c.

LETTER X.

New-York, *September 5th,* 1832.

Dear Sir,

I have endeavoured to communicate to you hitherto, such of the cases of Cholera Asphyxia, in which I have made or have witnessed post mortem examinations, as should best illustrate the pathologiac varieties which occur in that disease. Parallel cases would but encumber the Journal in which you have done me the honour of making the publication, and would afford no interest to yourself from the multiplicity which are now on record.

Through the continued politeness of my friend, Dr. Rhinelander, I assisted in the dissection of a subject at the Crosby-street Hospital this morning, whose case adds to the variety of morbid appearances which have fallen under my observation.

The subject was a labourer, about forty-five years of age, and his habits generally temperate. He was admitted yesterday in the stage of collapse; and after suffering severely the usual characteristic symptoms, died in the evening. The examination began about twelve hours after death. Surface natural, features contracted, muscles rigid, abdomen tumid. Stomach and small intestines inflated, the latter more than usually florid. Veins of the epiploon and mesentery full. Peritoneal investment adhesive to the touch. Mucous membrane of the stomach natural, of the small intestines slightly vascular, but the redness existed principally in the serous tissue. As is common in Cholera subjects, where redness of the small intestines occurs, it abounded most in the ilium. There was little else in the canal than a cream-like mucus, which was found in a preternatural quantity, but adhering to the membrane. The large intestines were contracted through their whole extent to about two thirds of an inch in diameter. The cœcum was also contracted in a corresponding degree. They of course presented a knotted appearance. The colour was healthy, and in every other respect they were perfectly natural. They contained nothing but mucus, less abundant than in the smaller portion, but very similar in sensible properties. This is the second instance only in which I have observed a contraction of the large intestines. The former I noticed in a subject at the Bellevue Hospital, and in that case the contraction was confined to the arch of the colon. This portion of the intestines is commonly found inflated—very rarely contracted. The liver was unusually pale, and no blood flowed from deep incisions. It could only be expressed from its largest veins. We could not detect a trace of bile in the organ ; it was otherwise healthy. The gall bladder contained about an ounce of yellowish bile—apparently two thirds filled. Urinary bladder entirely contracted. The other abdominal viscera natural. In the thorax, the lungs were very natural, and about two thirds exhausted of air. The left cavities of the heart were perfectly empty and natural ; the right contained a moderate quantity of blood, and were morbidly flabby. No petechiæ. Serous tissues of the parietes natural.

Dr. Rhinelander made a very minute dissection of the brain. It was rather more than usually soft, and its organization was distinct, and exhibited in a very interesting manner by Dr. R. The membranes were natural, and their veins contained less blood than is commonly found in subjects who have died of other diseases. The nerves, in their origin and extension, showed no mark of disease, nor could any be detected in the cerebral substance. A little florid blood exuded on the surface of the medullary portion. The ventricles contained their proper quantity of fluid, and the plexus was natural. The pineal and pituitary glands, and other parts, were found in their natural state. The cerebellum and medulla oblongata were equally free from marks of disease, and the basilary artery was empty.

In this subject, the oil of which I formerly spoke was found in small quantities on the blood, from whatever part examined.

No evidence of decomposition.

Very respectfully, &c.

THE END

A LECTURE

ON THE

PHYSIOLOGY OF DIGESTION,

INTRODUCTORY TO

A COURSE OF LECTURES

ON THE

INSTITUTES OF MEDICINE AND MATERIA MEDICA.

DELIVERED BEFORE THE

MEDICAL CLASS OF THE UNIVERSITY OF THE CITY OF NEW YORK,

AT THE SESSION OF 1844–5.

BY

MARTYN PAINE, A. M. M. D.,

Professor of the Institutes of Medicine and of Materia Medica in the University of the City of New York; member of the Royal Verein für Heilkunde in Preussen; of the Medical Society of Leipsic; of the Montreal Natural History Society, and other Learned Associations.

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1844–5.

New York, November 4, 1844.

Prof. Paine:

Dear Sir:—At a meeting of the Class of the University Medical College, held on Saturday evening last, Mr. S. S. Satchwell, of North Carolina, being in the chair, and Mr. Edmund R. Dabney, of Tennessee, acting as Secretary, on motion, it was unanimously

Resolved—That a Committee be appointed to solicit for publication, a copy of the able and eloquent Introductory Lecture delivered by Prof. Paine on Thursday evening last.

The undersigned having been appointed the Committee to perform this pleasing duty, allow us to add our individual wishes to those of the Class, and to hope that you will not refuse so unanimous a request.

We need not assure you how truly we are yours, &c.,

Philip A. Aylett, *Alabama.*
William J. Alexander, *Virginia.*
Cyrus Arndt, *New Jersey.*
John M. K. Alford, *North Carolina.*
John W. Albury, *West Indies.*
W. A. Burleigh, *Maine.*
J. Brown, *New Hampshire.*
E. T. Currie, *Maryland.*
G. W. Clippinger, *Pennsylvania.*
R. H. Clarke, *Delaware.*
Oliver Crooks, *Ohio.*
William Edwards, *Mississippi.*
G. G. Gaither, *Kentucky.*
W. M. Huntingden, *Vermont.*
B. S. James, *South Carolina.*
John McGregor, *Rhode Island.*
Charles T. Mount, *Canada.*
Valentine Mott, Jr., *New York.*
Edward Perry, *Connecticut.*
George W. Parkhill, *Forida.*
Wm. M. Ryer, *Illinois.*
J. B. Selby, *Wisconsin.*
George M. Tutt, *Georgia.*
T. D. Washburn, *Massachusetts.*
D. E. Warren, *Tennessee.*

New York, November 8, 1844.

Messrs. P. A. Aylett, *and others, composing a Committee of the Medical Class of the University of New York, &c.:*

Gentlemen:—In acknowledging the receipt of your very polite note, informing me of the honor done me by the Medical Class, of soliciting for publication a copy of my late Introductory Lecture, I have much pleasure in acceding to that request, and beg that you will convey to those gentlemen and accept for yourselves my sentiments of respect and esteem.

Very truly, and sincerely, yours,

MARTYN PAINE.

THE PHYSIOLOGY OF DIGESTION.

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1. "ANIMAL and VEGETABLE physiologists institute experiments without being acquainted with the circumstances necessary to the continuance of life,—with the qualities and proper nourishment of the animal or plant on which they operate,—or with the nature and chemical constitution of its organs. These experiments are considered by them as convincing proofs, whilst they are fitted only to awaken pity."

2. "ALL DISCOVERIES in physics and in CHEMISTRY, all explanations of CHEMISTS, [!] must remain without fruit and useless, because even to the GREAT LEADERS IN PHYSIOLOGY, carbonic acid, ammonia, acids, and bases, are sounds without meaning, words without sense, terms of an unknown language, which awaken no thoughts, and no associations. They treat these sciences like the vulgar, who despise a foreign literature in exact proportion to their ignorance of it."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

3. "None of them (the most distinguished physiologists) had a clear conception of the process of development and nutrition, or of the true cause of death. They professed to explain the most obscure *psychological* phenomena, and yet they were unable to say what fever is, and in what way quinine acts in curing it." The oft-reiterated conclusion follows, that IT IS RESERVED FOR CHEMISTRY TO RESOLVE THESE PROBLEMS.—LIEBIG'S *Animal Chemistry*. [See 1st and 2nd paragraphs p. 15, left hand column.]

4. "A writer, who can so contradict himself, scarcely needs to be exposed by us." —CARPENTER'S *Review of Paine's "Commentaries." See* PAINE'S "*Examination of Reviews*," pp. 12, 86.

5. "For many centuries past a profitless system has been followed, the same system that formerly obtained in natural philosophy, and the uncertainties and doubts of medical science are the best proofs of its value. It is the ruling principle of this system to satisfy the inquirer for facts by the use of empty words,—words which mean nothing and prove nothing. Life and vitality, with other sonorous epithets, figure away in these visionary speculations as though they were realities, and change their forms without reason or rule, as do the images that we see in dreams."

6. "Just in the same way that I am willing to admit the existence of forty different simple metals, so, upon similar evidence, I am free to admit the existence of fifty different imponderable agents, if need be. Is there anything which should lead us to suppose that the imponderables are constituted by Nature on a plan that is elaborately simple, and the ponderables on one that is elaborately complex? That the former are all modifications of one primordial ether, and the latter intrinsically different bodies, more than a quarter of a hundred of which have been discovered during the present century?"

7. "We are thus forced to admit that rays of light, rays of heat, *tithonic* rays, phosphoric rays, and probably many other radiant forms, have an INDEPENDENT EXISTENCE, and that they can be separated, by proper processes, from each other."—DRAPER'S *Treatise on the Forces which produce the Organization of Plants*, pp. 41, 70, 71.—New York, 1844.

8. "The inorganic and the organic world are blended together, depending both of them on common laws."—DRAPER'S *Introductory Lecture*, 1844—5.

9. "The rays of the sun are the true nervous principle of plants."—DRAPER'S *Treatise on the Forces*, &c. p. 102.

10. "As soon as the young plant has exposed itself to the solar beam, growth rapidly begins to take place, and organized matter to be condensed from the air," &c. —DRAPER'S *Introductory Lecture*.

11. "If, among plants light is the great agent of organization, electricity is the great motive force, which, under a specific modification, determines the movements of nutritious juices. The imponderable agents are the vital principle of organized systems."

12. "The oxygenizing action of the arterial blood is therefore the true cause of the systemic (general) circulation. [See 2nd paragraph, p. 11, left hand column.]

13. "The pulmonary circulation is therefore due to the oxydation of the venous blood."

14. "The old medical aphorism, '*ubi irritatio ibi affluxus*,' translated into the precise language of modern chemistry, simply means, *to the point where its deoxydation is taking place, the arterial blood will flow.*" [See 2nd paragraph, p. 11, left hand.]

15. "By the aid of the principles here laid down, all the various physiological or pathological conditions which are met with in inflammation, asphyxia, gangrene, &c. are

presented as so many interesting chemical problems for solution."—[See 4th paragraph, page 15th, right hand column.]

16 "It is a vulgar error that a living being possesses a principle of resistance to external agents, while a dead one submits itself to them. Both equally change, or of the two, the living one putrifies and changes the more rapidly."—DRAPER's *Treatise on the Forces*, &c. pp. 23, 36, 41, 6. [See 4th paragraph, p. 10, and 3rd p. 15, and line 5th, &c. p. 12, right column.]

17. "And now I hope to see this University, as it is the first to introduce these capital improvements, keep the first in the emulous race with all its sister institutions. I want to see it the great factory of Chemical discoveries and CHEMICAL DOCTRINES in the western world."—DRAPER's *Introductory Lecture.*

18. "Chemists and natural philosophers, accustomed to study the phenomena over which the physical forces preside, have carried their spirit of calculation into the theories of the vital laws."—*Bichat's General Anatomy*, vol. 2, p. 54.

19. "Let a man be given up to the contemplation of one sort of knowledge, and that will become every thing. The mind will take such a tincture from a familiarity with that object, that every thing else, how remote soever, will be brought under the same view. A metaphysician will bring ploughing and gardening immediately to abstract notions;—the history of nature will signify nothing to him. A chemist, on the contrary, shall reduce divinity to the maxims of his laboratory, explain morality by *sal, sulphur,* and *mercury,* and allegorize the Scripture itself, and the sacred mysteries thereof, into the philosopher's stone."—LOCKE, *on the Human Understanding.*

20. "Mr. Locke, I think, mentions an eminent musician, who believed that God created the world in six days, and rested on the seventh, because there are but seven notes in music. I, myself, knew one of that profession who thought there were only three parts in harmony, to wit: base, tenor, and treble, because there are but three persons in the Trinity."—REID, *on the Powers of the Human Mind, vol.* 2 *Essay* 6. *c.* 8.

21. "When education takes in error as a part of its system, there is no doubt that it will operate with abundant energy, and to an extent indefinite."—*Burke.*

DIFFERENCES of opinion on questions of great moment to mankind are apt to be strongly conveyed, and apparent error to be censured in no measured terms. This, perhaps, is often admissible, considering the obstinacy of error, and so long as it is the doctrine, and not its author, which is assailed. We may revere the names of Voltaire, of Hume, and of Gibbon, yield them a proud rank in the scale of intellect, and gratefully acknowledge the rich legacies they have left behind. But, who of us would hesitate to speak of their infidelity according to its nature and tendencies? This is even demanded by what we believe of the precepts of religion. And so of the principles of medicine, which hold as high a relation to the temporal interests of man, as do the precepts of religion to his spiritual welfare. The highest order of intellect is often devoted to the dissemination of error, and perhaps more frequently in religion and medicine than in any other of the great interests of mankind. This must be fully and firmly met, not only by evidences of the truth, but by an exposure of its perversions and corruptions.

In the discussion of my subject, (the controverted physiology of digestion,) I shall bring to your consideration the misapplication of chemistry to the processes of life; but in doing this, it will be only in the defence of truth, and not with a view of diminishing your respect for an important science. We must regard its defects abstractedly, and endeavor to eradicate such impurities as tarnish its lustre. We must still yield to it all the praise to which it is entitled by the devoted industry of its cultivators, by its elevated rank amongst the sciences, and by its usefulness to man. Indeed, as a science of general usefulness, as a source of mental improvement and of pure and elevated enjoyment, as opening to our view many of the most deeply interesting and stupendous institutions of nature, and often with that demonstration which no skepticism can resist,—I say, with all these combined advantages, it is the most important of the sciences for the general purposes of life. And I am mostly prompted in making these remarks by the too common neglect of chemistry by the medical profession, to whom it is farther recommended by its subserviency to the healing art.

At the outset of my subject, it will be necessary to premise a few statements as to the general condition of medicine, and the auspices under which it is now cultivated.

The physiological world, as you are probably aware, has been lately divided into three schools. One of these sects virtually regards organic nature as a part only of inorganic, endowed with the same properties and governed by the same laws. It maintains, in short, that there is no real difference between a man and a stone. At the head of this school stands Liebig, the distinguished and able chemist. It is a great and powerful school, but is falling, daily, beneath the weight of its vast errors and corruptions. It is denominated the CHEMICAL SCHOOL of medicine. Contrasted with this is the school of VITALISM; and to the interests of this school, I may now say, the main efforts of my life have been devoted. Their doctrines will be taught in the lectures which I shall have the honor of laying at the foundation of your medical education; not, however, with any concealment of adverse opinions, but with an ample exhibition of their nature, and of their claims to your consideration. This school regards organic and inorganic nature as distinct in their most essential attributes; that each department is governed by properties and laws peculiar to itself. It regards the organic being as fundamentally distinct from the inorganic in its elementary constitution, in the aggregation of its molecules, in the structure of its parts, in its condition as a whole, and in every phenomenon which it evinces. It sees design in every part of the living being, eloquent even in the dry bones of a skeleton; a design peculiar to every part, whilst all concur in harmony together to the common ends of one universal design. On the contrary, also, this school discerns little of the nature of design in the constitution, or in the abstract condition of inorganic matter. It sees nothing, in respect to power, but mere *vis inertiæ;* which, however, is supposed by the physical school to be capable of evolving from simple matter every variety of organization, with all its specific designs, even instinct and reason, whilst at the same time we hear from the depth of materialism, that "organic nature is the mystery of mysteries."

Again, the vitalists, in consideration of the general facts now stated, maintain, in the language of Liebig, the great head of the school of mere physics, "the existence of a POWER distinct from all other powers of nature, namely, a VITAL PRINCIPLE;" which organizes and governs all living beings, and which is the fundamental cause of all their phenomena in health and disease. I say, in the language of Liebig, "a power distinct from all other powers of nature, namely, *a vital principle;*" for this mere chemist, in his conflicts with living nature, concedes the existence of such a principle as at the foundation of all vital phenomena, yet in the same general manner, and on all specific questions where he has introduced its direct and exclusive agency, he as unequivocally declares that there is no such principle, and that every result of life, and disease, even thought itself, are entirely owing to chemical agencies. His whole system, as set forth in his "Organic Chemistry applied to Physiology," and in his "Animal Chemistry" applied to Pathology and Therapeutics, is a tissue of similar contradictions, of the boldest assumptions. and of stultifying doctrines. Yet, with deep mortification I say it, he has been hailed with an enthusiasm before unknown in the annals of medicine, as the only true expounder of physiology and of medical philosophy. The world, however, is fast awaking from its spellbound delusion, and the doctrines of this "reformer" will soon be mingled with the same and more original chimeras which did their part in "the dark ages of science."

Finally, the third school, or that of CHEMICO-PHYSIOLOGY, endeavor to form, as it were, a bond of union between the schools of pure vitalism, and of pure chemistry, though such an alliance be as unnatural as human brains in a block of granite. The chemico-physiologist makes a compromise with philosophy, and takes for his rule, "*in medio tutissimus ibis.*" He professes to loathe all extremes; but takes little trouble to ascertain the fact whether one doctrine or the other be fairly entitled to this appellation. He sees only the direct opposition which exists, without caring whether one "extreme," as he calls it, may not be perfectly true, whilst the other is as perfectly false. He therefore mingles the doctrines of vitalism and of chemistry; allotting to the former one half of the phenomena of life, and the other half to the latter. This is the school to which you have the greatest chance of becoming the victims; for it is apparently recommended by the conciliatory principle which I have stated in the form of its motto, and by many of the most distinguished members of our profession.

I am now prepared by these explanatory remarks, relative to the three schools of medicine, to enter upon the consideration of the philosophy of digestion; for each denomination have interpreted that philosophy according to the general doctrines of life which are peculiar to each.

Beginning with pure chemistry, we find the great leader setting forth the process of digestion in the following language, in his late work on Animal Chemistry applied to Pathology and Therapeutics. "CHYMIFICATION," he says, "IS INDEPENDENT OF THE VITAL FORCE. It takes place in virtue of A PURELY CHEMICAL ACTION,—*exactly similar* to those processes of decomposition and transformation which are known as *putrefaction*, *fermentation*, or *decay*." This is exactly the old doctrine of what are well denominated the "dark ages."

Now you will recollect that I have stated that it is a remarkable characteristic of the work which is assumed as the basis of the chemical philosophy of life, that it abounds with staring contradictions; and such is the fact in relation to the doctrine which I have just quoted. And how could it be otherwise, seeing that its author was constantly employed about two subjects that have no relation to each other; that is to say, the philosophy of life and the philosophy of chemistry. But I will go back, for a conflicting doctrine, to the "Reformer's" treatise on Organic Chemistry applied to Physiology, published a year or two antecedently to his work on Animal Chemistry; by which we shall learn the extent of the confusion which pervades his writings, and the tardiness with which it is discerned by his medical disciples.

In that work, he says, "the equilibrium in the chemical attractions of the constituents of food is disturbed BY THE VITAL PRINCIPLE. The UNION of its ELEMENTS, so as to produce *new combinations and forms*, indicates the presence of A PECULIAR MODE OF ATTRACTION *and the existence of* A POWER DISTINCT FROM ALL OTHER POWERS OF NATURE, namely, THE VITAL PRINCIPLE." "If the food possessed life, not merely the chemical forces, but THIS VITALITY would offer resistance to the *vital force* of the organism it nourished."

Such, then, was the opinion of this Chemist when discoursing of life in his "Organic Chemistry applied to Physiology;" and passages of a similar import occur in his late work on "Animal Chemistry,"—ay, and standing in connection with his equally fundamental, and hostile doctrine, which refers every living process and result to the merest chemical agencies.

The work, however, on Animal Chemistry is more of a distillation from the Laboratory than its predecessor; and as many of the most eminent physiologists in Europe, who were inclined to mingle chemistry with vitalism, were nauseated by the dose which was last administered, Liebig came out in his lectures for the session of the present year, (1844) with the following placebo for the vitalists and the chemico-physiologists. It is of some extent; but, as it embraces exactly the doctrines which I have been always employed in teaching, and which are still fundamental in my lectures, I shall quote it, at length; so that, should there be any present who may have become imbued with the popular superstition in regard to Liebig's notions in Physiology, they may rest assured that my lectures will have for their basis what will be allowed by their favorite Liebig to be the only true foundation of the Institutes of Medicine. Thus, then, the German Chemist, when, only six months ago, he was lecturing, like myself, on the functions of life.

"After the EXTINCTION* of the vital Principle," he says, "in organic atoms, they maintain their form and properties, the state into which they have been brought in living organisms, *only* by reason of their *inherent inertia*. It is a great and comprehensive law of matter that its particles possess no self-acting, no inherent power of originating motion, when at rest. Motion must be imparted by some external cause; and, in like manner, motion once imparted to a body can only be arrested by external resistance." So much for simple matter. Now for matter in an organic state. "The constituents of vegetable and animal substances," he goes on, "having been formed under the guidance and power of *the vital principle*, it is *this principle* which determines the direction of their molecular attraction. THE VITAL PRINCIPLE, therefore, must be A MOTIVE POWER, CAPABLE OF IMPARTING MOTION TO ATOMS AT REST, and of *opposing resistance* to OTHER FORCES producing motion, such as the CHEMICAL FORCE, heat and electricity. We are able to reliquify and redissolve albumen after it has been coagulated by heat; but, the vital principle ALONE is capable of restoring the original order and manner of the molecular arrangement in the smallest particles of albumen. Coagulated albumen is again converted into its original form ; it is transformed into flesh and blood in the animal organism.

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* See my "*Examination of Reviews*," p. 26—28.

"In the formation of vegetable and animal substances the vital principle opposes, as a FORCE OF RESISTANCE, the action of the OTHER FORCES,—cohesive attraction, heat, and ELECTRICITY,[*]—forces which render the aggregation of atoms into combinations of the *highest* order IMPOSSIBLE, *except in living organisms.*

"Hence it is, that when those complex combinations which constitute organic substances are withdrawn from the influence of the VITAL FORCE,—when this no longer is opposed to the action of the OTHER disturbing forces, great alterations immediately ensue in THEIR PROPERTIES, and IN THE ARRANGEMENT of their constituents. The slightest chemical action, the mere contact of atmospheric air, suffices to cause a transposition in their atoms, and to produce new arrangements ; in one word, to excite DECOMPOSITION. Those remarkable phenomena then take place which are designated by the terms *fermentation*, *putrefaction*, and *decay*. THESE are the processes of DECOMPOSITION, and their ultimate results are to reconvert the elements of organic bodies into that state in which they exist *before they participate in the processes of life.*" —*London Lancet, May* 18, 1844.

Now, gentlemen, you are no doubt astonished, and well you may be, to hear such language, and the latest, too, that he has spoken, from the chief of the school who, IN REALITY, admit of nothing but pure chemistry in their system of physiology. The language which I have just quoted from Liebig, should place him amongst the unqualified vitalists. It conveys the doctrines which lie at the foundation of my Medical and Physiological Commentaries, and which are opposed, *in toto*, to the hallucinations of Chemistry. And yet I have already made other quotations from the same writer, and on the same subjects, which you will have observed contradict, in the broadest and most absolute manner his doctrines as to the vital principle. The latter seem to be thrown out "as springes to catch woodcocks," or as others have it, as "a tub to the whale."

Such, then, are examples of the medley of contradictions in great fundamental principles, with which the writings of this very extraordinary and successful pretender in medicine, abound. On the same page we meet a purely chemical and a purely vital philosophy of digestion ; and equally so of other important organic processes. Each is laid down without qualification, and with the *dictum* of a master who is conscious that the preponderance he gives to the purely chemical philosophy of life will establish his Empire in that philosophy with an age more prone than ever to the doctrines of materialism. On the very subject of a *vital principle* itself he is as flatly contradictory as on the cause of digestion ; for at one moment he avows the existence of such a principle, "distinct from all other powers of nature," and calls it "the vital principle," which he says governs all the processes of living beings, and at the next moment he avers that "IN THE ANIMAL BODY WE RECOGNIZE, AS THE ULTIMATE CAUSE OF ALL FORCE, ONLY ONE CAUSE, THE CHEMICAL ACTION *which the elements of the food and the oxygen of the air mutually exercise on each other. The only known ultimate cause of vital force, either in animals or in plants*, IS A CHEMICAL PROCESS." Thus, also, he increases the medley of contradictions in relation to digestion, by making that process to depend on a purely chemical action, and to evolve that vital principle which he had already avowed is the only power concerned in chymification.

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[*] *Heat, light*, and *electricity*, are often absurdly supposed to constitute *forces* in their relation to living beings. In that relation they are merely *vital stimuli*, calling into action the properties of life. These properties are the only forces which have any connection with the phenomena evinced by animals or plants. As to the doctrine of *capillary attraction*, whether applied to the capillary circulation of plants or animals, it is the very worst of all the physical views of life ; since it strikes a fatal blow at all organic processes and results, and at all the physiological and pathological doctrines which are founded upon them. The doctrine, on the other hand, is contradicted by all the phenomena and results which appertain to organic life ; since they are all dependent on special modes of vital action carried on by the capillary system.

But be not deceived; for, however this "reformer" may beguile you with words, and seem to persuade rather than to rule, remember that, at most, he does but invalidate his own edicts by countermands, and that in the end he tells you that those apparently adverse decrees are, in their absolute import, one and the same; that they are consistent laws delivered from the laboratory, though apparently in conflict on account of the opposing forces, the attraction and repulsion, which preside in the chemistry of nature; that, however, in reality, there is no difference whatever in the seemingly two great principles which lie at the foundation, which are one and identical, since "THE MYSTERIOUS VITAL PRINCIPLE CAN BE REPLACED BY THE CHEMICAL FORCES,"* and since, also, "THE VITAL FORCE UNITES IN ITS MANIFESTATIONS ALL THE PECULIARITIES OF THE CHEMICAL FORCES, AND OF THE NOT LESS WONDERFUL CAUSE WHICH WE REGARD AS THE ULTIMATE ORIGIN OF ELECTRICAL PHENOMENA."† ! !

I dwell upon these absurdities, that you may the better realize the shallowness of that pretended philosophy which has so lately swept, like a hurricane, over the intellectual world; that you may see, in this system of contradictions, the equal fallacy of that school who endeavor, with greater sincerity, to mingle the conflicting principles, and that you may the better cultivate and enjoy the simple and consistent philosophy which nature teaches. Nor will I yet leave that stupendous system of assumption and contradiction which was so lately hailed by physiologists as the harbinger of a total revolution in medical science, ay, in the very practice of medicine, without showing you the depth of the materialism in which it was submerged. I say nothing now of the avowed infidelity to which it has led. Examples of that disregard of instinctive faith I have already placed in their proper connection with my present subject.‡ But, I will merely quote from Liebig's revolutionary work a doctrine of the chemical school, from which, if I mistake not your ambition as intellectual and immortal beings, the very impulse of nature will turn you with a loathing aversion. You will see from it, also, how utterly degraded to the rank of the merest matter is every thing relating to organic life; even man himself. Thus, then, our author in behalf of the school of chemistry. "Physiology," he says, "has sufficiently decisive grounds for the opinion, that *every motion, every manifestation of force*, is the RESULT of a transformation of the structure or of its substance; that every conception, every mental affection, is followed by changes in the chemical nature of the secreted fluids; that every thought, every sensation, is accompanied by a change in the COMPOSITION of the substance of the brain."—"*Every manifestation of force* is the RESULT of a transformation of the structure or of its substance."

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* LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

† LIEBIG'S *Animal Chemistry.*

‡ See my "*Medical and Physiological Commentaries,*" vol. 2, p. 122—140. Also the *Essay on the Vital Powers.*

Many organic chemists, however, are disposed to admit the existence of a spiritual part, and they should therefore recollect that the existence of a principle of life is substantiated by far more numerous facts than the spirituality of mind, which they are so ready to concede when inviting your attention to their physical doctrines of life. They should be as ready to yield to one series of facts as to the other, and especially to the more cogent series when the minor is *obtruded* upon your belief.

Let us now observe the hostile attitude of some of the important doctrines which I have quoted from Liebig, by repeating them in direct connection, that we may the more distinctly realize the absurdities which come to us from the laboratory of the chemist, when its ambitious aim is turned upon physiology. Examples of similar conflicting views abound in the writings of "the reformer."—Thus:—

"MY OBJECT has been, in the present work, to direct attention to the points of intersection of *chemistry with physiology*, and to point out those parts in which the sciences become, as it were, *mixed up together*. It contains a collection of problems, such as *chemistry* at present requires to be resolved, and a number of conclusions drawn *according to the rules* of that science. These questions and problems will be resolved; and we cannot doubt that we shall have in that case A NEW PHYSIOLOGY AND A RATIONAL PATHOLOGY."—LIEBIG'S *Animal Chemistry.*

"In the *animal body* we recognise as the ultimate *cause of all force* only *one cause*, the CHEMICAL ACTION which the elements of the food and the oxygen of the air mutually exercise on each other. The only known ultimate *cause* of vital force, either in animals or in plants, is A CHEMICAL PROCESS. If THIS be *prevented*, THE PHENOMENA OF LIFE DO NOT MANIFEST THEMSELVES. If the *chemical action* be *impeded*, the vital phenomena must take *new forms*" "ALL VITAL ACTIVITY ARISES from the mutual action of the oxygen of the atmosphere and the elements of the food."—LIEBIG'S *Animal Chemistry.*

"Physiology has sufficiently decisive grounds for the opinion, that EVERY MOTION, EVERY MANIFESTATION OF FORCE, IS THE RESULT OF A TRANSFORMATION OF THE STRUCTURE OR OF ITS SUBSTANCE; that *every conception*, *every mental affection*, is followed by *changes* in the chemical nature of the secreted fluids; that *every thought*, *every sensation*, is accom-

"A RATIONAL physiology cannot be founded on mere re-actions, and the living body cannot be viewed AS A CHEMICAL LABORATORY."—LIEBIG'S *Animal Chemistry.*

"The study of the USES of the functions of different organs, and of their MUTUAL CONNECTION in the animal body, was formerly the chief object in physiological researches; but lately THIS STUDY has fallen into the background."—*Ibid.* [See 3rd motto.]

"In the *animal ovum*, as well as in THE SEED OF A PLANT, we recognize A CERTAIN REMARKABLE FORCE, THE SOURCE OF GROWTH, or increase in the mass, and of reproduction, or of supply of the matter consumed; A FORCE IN A STATE OF REST.* By the action of external influences, by impregnation, by the presence of air and moisture, the condition of STATIC EQUILIBRIUM of this force is disturbed. ENTERING INTO A STATE OF MOTION OR ACTIVITY, *it exhibits itself* IN THE PRODUCTION *of a series of forms*, which, although occasionally bounded by right lines, are yet widely distinct from geometrical forms, such as we observe in crystallized minerals. This force is called THE VITAL FORCE, *vis vitæ*, OR VITALITY."—LIEBIG'S *Animal Chemistry.*—[See my *Essays on Vitality*, &c. p. 13—18.]

"The VITAL PRINCIPLE must be a MOTIVE POWER, CAPABLE OF IMPARTING MOTION TO ATOMS AT REST, and of OPPOSING panied by a change in the *composition* of the substance of the brain"!—LIEBIG'S *Animal Chemistry.*

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* See my "*Examination of Reviews*,' p. 7—24.

"In the processes of NUTRITION and REPRODUCTION, we perceive the passage of matter from the state of motion to that of rest, (static equilibrium.) Under the influence of the nervous system, this matter enters again into a state of motion. The ultimate causes of these different conditions of the vital force are CHEMICAL FORCES."

"THE CAUSE of the state of MOTION IS to be found in a series of changes which the food undergoes in the organism, and these are the RESULTS OF PROCESSES OF DECOMPOSITION, to which either the food itself, or the STRUCTURES formed from it, or parts of organs, are subjected."—LIEBIG'S *Animal Chemistry.* [See 6th and 7th paragraphs, page 13, left hand column, and right hand column, page 11–12, and 12th, 13th, and 14th mottos.

This summary principle, were it true, would be truly beautiful. But it appears to be not only deficient in every necessary element, but to be contradicted by all the phenomena of sympathy and by all that is known of pathology and therapeutics; and this, too, according to the author's own repeated and fullest affirmations and doctrines to the contrary.]

"THE POWER to effect TRANSFORMATIONS *does not belong to the vital principle.* Each transformation is owing to a disturbance in the attraction of the elements of a compound, and is, consequently, A PURELY CHEMICAL PROCESS."—LIEBIG'S *Organic Chemistry applied to Physiology,* &c.—[See, also 1st paragraph, page 14, and 6th paragraph, page 14, right hand column, and others equally contradictory.]

[I occupy this space to indicate to the reader that he will find that Liebig considers the "chemical action which the elements of the food and the oxygen of the air mutually exercise on each other," to be the effect of life and the cause of life, and the cause of death, and the cause of animal heat, and that respiration is the essential cause of the whole,—even of death itself. Thus we have all the phenomena of organic beings, from the cradle to the grave, referred to a single chemical process.]

"Every thing in the animal organism, to which the name of MOTION can be applied, proceeds from the NERVOUS apparatus. The phenomena of MOTION IN VEGETABLES, the circulation of the sap, for example, observed in many of the characeæ, and the closing of flowers and leaves, depend on physical and mechanical causes. HEAT and LIGHT are the REMOTE RESISTANCE to OTHER forces producing motion, such as THE CHEMICAL FORCE, heat and ELECTRICITY."—LIEBIG'S *Lectures for* 1844. [See 16th motto.]

"In the animal organism we are acquainted with ONLY ONE CAUSE OF MOTION; and this is the SAME CAUSE which *determines the growth* of living tissues, and *gives them the power of resistance* to external agencies. IT IS THE VITAL FORCE."

"There is NOTHING to prevent us from considering the VITAL FORCE as a PECULIAR property, which is possessed by certain material bodies, and becomes sensible when their elementary particles are combined in a certain arrangement or form. This supposition takes from the *vital phenomena* nothing of their wonderful peculiarity. It may, therefore, be considered as a RESTING POINT from which an investigation into these phenomena, and the LAWS which regulate them, may be commenced; exactly as we consider the properties and laws of LIGHT to be dependent on a certain *luminiferous matter* or other, which has no farther connection with the laws ascertained by investigation."—LIEBIG'S *Animal Chemistry.*

"Our notion of life involves something more than mere reproduction, namely, the idea of AN ACTIVE POWER exercised by virtue of a definite form, and production and generation in a definite form. The production of organs, and their power not only to produce their component parts from the food presented to them, but to GENERATE THEMSELVES *in their original form* and *with all their properties*, are characters BELONGING EXCLUSIVELY TO ORGANIC LIFE, and constitute a form of reproduction INDEPENDENT OF CHEMICAL POWERS. The chemical forces are subject TO THE INVISIBLE CAUSE BY WHICH THIS FORM IS PRODUCED. OF THE EXISTENCE OF THIS CAUSE ITSELF we are made aware ONLY *by the phenomena* which IT PRODUCES. ITS LAWS must be investigated *just* as we investigate THOSE of the OTHER POWERS which effect motion and changes in matter."

"THE VITAL PRINCIPLE is ONLY known to us through the peculiar form of ITS INSTRUMENTS; that is, through the organs IN WHICH IT RESIDES. Hence, whatever kind of energy a substance may possess, if it is amorphous and destitute of ORGANS from which the IMPULSE, MOTION, or change, proceeds, IT DOES NOT LIVE. Its ENERGY depends, in THIS CASE, on a CHEMICAL ACTION. LIGHT, HEAT, electricity, or other

CAUSES of MOTION in VEGETABLES; but in ANIMALS we recognize in the *nervous apparatus* A SOURCE OF POWER, capable of renewing itself at every moment of their existence "—Liebig's *Animal Chemistry*. —[See, also, 3rd paragraph, page 10, right hand column.]

Nevertheless, although—

"Plants require LIGHT, and, indeed, sun light, it is not necessary that the direct rays of the sun reach them. Their FUNCTIONS certainly proceed with greater intensity and rapidity in sunshine, than in the diffused light of day; but it merely ACCELERATES in a greater degree THE ACTION ALREADY EXISTING."—Liebig's *Organic Chemistry applied to Physiology*, &c. [See note p. 8, and opposite paragraph.]

"THE VITAL FORCE UNITES in its manifestations ALL THE PECULIARITIES OF CHEMICAL FORCES, and of the not less wonderful CAUSE which we regard as the ultimate origin of ELECTRICAL phenomena."—Liebig's *Animal Chemistry*. [See, also, 4th paragraph, page 10, right hand column, and first paragraph, page 8, and *note*.]

"The mysterious VITAL PRINCIPLE can be REPLACED by the CHEMICAL FORCES."—Liebig's *Organic Chemistry applied to Physiology*, &c.

"While the ASSIMILATION of food in VEGETABLES and the WHOLE PROCESS OF THEIR FORMATION, are DEPENDENT on certain EXTERNAL INFLUENCES *which produce motion*, the *development* of the ANIMAL organism is, to a certain extent, *independent* of those external influences, just BECAUSE the ANIMAL body can produce WITHIN ITSELF THAT SOURCE OF MOTION WHICH IS INDISPENSABLE TO THE VITAL PROCESS."—Liebig's *Animal Chemistry*.

"Neither the emission of carbonic acid nor the absorption of oxygen (by *plants*) has any connection with the process of ASSIMILATION; nor have they the slightest relation to each other. The one is *purely a mechanical*, the other a *purely chemical process*. A COTTON WICK, inclosed in a lamp, which contains a liquid *influences* [justly considered *here* by Liebig as vital *stimuli* and *not forces*] may increase, diminish, or arrest THIS ACTION; but they ARE NOT ITS EFFICIENT CAUSE." "THE VITAL PRINCIPLE OPPOSES to the continual action of the atmosphere, moisture, and temperature, upon the organism, A RESISTANCE which is, in a certain degree, *invincible*. It is by the *constant neutralization* and *renewal* of these external influences that *life and motion are maintained*."—Liebig's *Organic Chemistry applied to Physiology*, &c.

"IN WHAT FORM OR IN WHAT MANNER THE VITAL FORCE PRODUCES MECHANICAL EFFECTS IN THE ANIMAL BODY IS ALTOGETHER UNKNOWN, AND IS AS LITTLE TO BE ASCERTAINED BY EXPERIMENT AS THE CONNECTION OF CHEMICAL ACTION WITH THE PHENOMENA OF MOTION, which we can produce with the galvanic battery. We know not how a *certain invisible something, heat*, gives to certain bodies the power of exerting an enormous pressure on surrounding objects. We know not even how *this something* itself is produced when we burn wood or coals.

"So it is with THE VITAL FORCE, and with THE PHENOMENA exhibited by living bodies. The CAUSE of these PHENOMENA IS NOT CHEMICAL force; IT IS NOT ELECTRICITY, nor magnetism. It is a PECULIAR FORCE, because it exhibits manifestations which are formed by NO OTHER KNOWN FORCE."

"In regard to the NATURE AND ESSENCE of the vital force, we can hardly deceive ourselves, when we reflect, that it behaves, in all its manifestations, exactly like OTHER natural forces; that it is devoid of consciousness or of volition, and is *subject to the action of a* BLISTER."—Liebig's *Animal Chemistry*.

"An abnormal production of certain component parts of plants *presupposes* A POWER AND CAPABILITY OF ASSIMILATION, to which the most powerful CHEMICAL ACTION CANNOT BE COMPARED. The best idea of it may be formed, by considering that it surpasses in power the strongest galvanic battery, with which we are not able to separate the oxygen from carbonic acid, as is done by the *leaves of plants*," "and without the direct solar rays."

"All that we can do is to supply those substances which are adapted for ASSIMILATION BY THE POWER *already present* in the organs of the plant."—Liebig's *Organic Chemistry applied to Physiology*, &c. —See, also, third quotation in this column, page 10.

"The living part of a plant acquires the whole force and direction of its VITAL saturated with carbonic acid, acts *exactly* in the same manner as a living plant in the night."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"At night, a true CHEMICAL process COMMENCES, [though EVERY process is either *purely mechanical* or *purely chemical*, by *night* or by *day*, according to the last quotation] in consequence of the action of the oxygen of the air upon the substances composing the leaves, blossoms, and fruit. This process is not at all connected with the life of the vegetable organism, because it goes on in the DEAD plant EXACTLY as in a *living* one."!

Nevertheless,—

"What value can be attached to experiments, in which all those matters which *a plant* REQUIRES in the process of ASSIMILATION, besides its MERE NUTRIMENT, have been EXCLUDED WITH THE GREATEST CARE? Can the LAWS OF LIFE be investigated in an organized being which is diseased or DYING?"—LIEBIG'S *Organic Chemistry applied &c*—Or, can those laws be investigated in "*a Cotton wick,* inclosed in a lamp"?

"Analogy, that fertile source of error, has unfortunately led to the very UNAPT COMPARISON of the VITAL functions of PLANTS with those of ANIMALS."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.—[See, also, right hand column p. 12.]

Plants, when properly placed in *glass* vessels containing distilled water, will, by means of their ROOTS, DECOMPOSE the glass, and *absorb its silex and alkali*, and *assimilate* those substances to themselves, and in despight of any chemical affinities; yet,

"All substances in solution in a SOIL are absorbed by the ROOTS of plants, EXACTLY as a sponge imbibes a liquid, and all that it contains, WITHOUT SELECTION," and "their ASSIMILATION is a PURELY CHEMICAL PROCESS."—LIEBIG'S *Organic Chemistry applied*, &c.,

"The most decisive experiments of physiologists have shown that the process of CHYMIFICATION *is independent of the* vital force; that it takes place in virtue of a PURELY CHEMICAL action, EXACTLY SIMILAR to those processes of DECOMPOSITION or transformation which are known as PUTREFACTION, FERMENTATION, or DECAY."—LIEBIG'S *Animal Chemistry.*

"Those remarkable phenomena, FERMENTATION, PUTREFACTION, and DECAY, are THE PROCESSES of DECOMPOSITION, and their ultimate results are to RECONVERT the elements ENERGY from the absence of all conductors of force. BY *this means* the *leaf* is enabled to overcome the *strongest chemical attractions*, to DECOMPOSE CARBONIC ACID, and to ASSIMILATE the ELEMENTS of its nourishment."—LIEBIG'S *Animal Chemistry.*

"In vegetable physiology, a leaf is regarded in every case *merely as a leaf,* notwithstanding that leaves generating oil of turpentine or oil of lemons, *must possess a different nature* from those in which oxalic acid is formed. VITALITY, in its *peculiar operations,* MAKES USE of a SPECIAL apparatus for *each* function of an organ. Vegetable physiologists, in the study of their science, have not directed their attention to *that part* of it (the laws of vitality) which is *most worthy* of investigation."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"In the living plant, the INTENSITY of the VITAL FORCE far exceeds that of the CHEMICAL ACTION of oxygen. We know, with the UTMOST CERTAINTY, that, by the influence of the VITAL FORCE, OXYGEN is separated FROM ELEMENTS to which it has the strongest affinity; and that it is given out in the gaseous form, without exerting the slightest action on the juices of the plant."

"THE ANIMAL ORGANISM IS A HIGHER KIND OF VEGETABLE."

"ASSIMILATION, or the process of FORMATION and GROWTH, goes on in the SAME WAY in ANIMALS and in VEGETABLES. In both the SAME CAUSE determines the increase of mass. This constitutes the TRUE vegetative life."—LIEBIG'S *Animal Chemistry.*

"The CONSTITUENTS of VEGETABLE and animal substances *are formed* under the guidance and power of THE VITAL PRINCIPLE, which *determines* the direction of their molecular *attraction*." "In the formation of *vegetable* and animal substances, THE VITAL PRINCIPLE opposes, as a FORCE OF RESISTANCE, the action of the OTHER FORCES," &c.—LIEBIG'S *Lectures for* 1844.

"The EQUILIBRIUM in the chemical attractions of the constituents of food is disturbed BY THE VITAL PRINCIPLE"; and "THE UNION of its ELEMENTS, so as to produce *new combinations* and *forms*, indicates *a peculiar mode of attraction*, and the existence of A POWER DISTINCT FROM ALL OTHER POWERS OF NATURE, namely, the VITAL PRINCIPLE."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"The VITAL FORCE CAUSES A DECOMPOSITION of the constituents of food, and de-ments of organic bodies into that state in which they exist before they participate in the processes of life."—LIEBIG'S *Lectures for* 1844.

"In the action of the gastric juice on the food, NO OTHER element takes a share except the OXYGEN of the atmosphere." "This air by means of the saliva, reaches the stomach with the food, and there its oxygen enters into combination."—LIEBIG'S *Animal Chemistry*.

"THE POWER of elements to unite together, and to form peculiar compounds which are generated in animals and vegetables, is CHEMICAL AFFINITY."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c [See, also, as to *elements*, the 6th, page 12, and 2d paragraph, page 13, and 3rd paragraph, page 10, and the 3rd, 4th, 5th, and 6th paragraphs, page 14, right hand column; and other places.]

"We should not permit ourselves to be withheld, by the idea of a VITAL PRINCIPLE, from considering in a CHEMICAL POINT OF VIEW, the process of TRANSFORMATION OF THE FOOD, and its ASSIMILATION by the VARIOUS ORGANS. This is the more necessary, as the views, hitherto held, have produced no results, and are quite incapable of useful application."—LIEBIG'S *Organic Chemistry applied* &c.—[See third paragraph, page 11, right hand column, and other contradictory quotations in same column.]

"In the natural state of the digestive process, the food *only undergoes a change in its state of cohesion*, becoming fluid without any other change of properties." —LIEBIG'S *Animal Chemistry*.

Although—"the process of CHYMIFICATION IS INDEPENDENT of the *vital force*, and takes place in virtue of a *purely chemical action*, EXACTLY *similar* to those processes of Decomposition which are known as PUTREFACTION, FERMENTATION, OF DECAY";—nevertheless, "INORGANIC compounds DIFFER from ORGANIC *in as great a degree* as in their SIMPLICITY *of constitution*."—LIEBIG'S *Animal Chemistry*, and *Organic Chemistry*. [See next quotation, this column, and 1st and 2d paragraphs page 22.]

"By CHEMICAL AGENCY we can produce the CONSTITUENTS of muscular fibre, skin, and hair"! "We are able to form, in stroys the force of attraction which is continually exerted between their molecules. It alters the direction of the CHEMICAL FORCES in such wise, that the ELEMENTS of the constituents of the food arrange themselves in another form, and combine to produce *new compounds*. It FORCES the *new compounds* to assume forms ALTOGETHER DIFFERENT from those which are the result of the attraction of cohesion when acting freely, that is, without resistance."

"The VITAL FORCE appears as a MOVING FORCE or cause of motion, when it OVERCOMES the CHEMICAL FORCES, cohesion and AFFINITY, which act between the constituents of food, and when IT CHANGES the position or place in which their ELEMENTS occur. The VITAL FORCE is manifested as A CAUSE OF MOTION in OVERCOMING THE CHEMICAL ATTRACTION of the constituents of food, and is, farther, THE CAUSE WHICH COMPELS them to combine in a new arrangement, and to assume new forms."—LIEBIG'S *Animal Chemistry*.

"When a chemical compound of simple constitution is introduced into the stomach, ITS CHEMICAL ACTION is, of course, OPPOSED BY THE VITAL PRINCIPLE. The results produced depend upon the strength of their respective actions. Either an *equilibrium* of BOTH POWERS is attained, a change being effected without the *destruction of the vital principle*; in which case a MEDICINAL EFFECT is occasioned. Or, the acting body YIELDS TO THE SUPERIOR FORCE OF VITALITY, that is, IT IS DIGESTED. Or, lastly, the CHEMICAL ACTION obtains the ascendency and ACTS AS A POISON."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"The individual organs, such as the STOMACH, cause all the organic substances conveyed to them, which are capable of transformation, to assume NEW FORMS. The STOMACH *compels* the ELEMENTS of these substances to unite into a COMPOUND *fitted* for the formation of the blood."—LIEBIG'S *Organic Chemistry*, &c.

"The FIRST substance capable of affording nutriment to animals is the LAST product of the CREATIVE ENERGY of vegetables."—LIEBIG'S *Animal Chemistry*.

"The special characters of food, that is of substances fitted for assimilation, are ABSENCE OF ACTIVE CHEMICAL PROPERTIES, and the capability of yielding to transformations."—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"The CONSTITUENTS of VEGETABLE and ANIMAL substances HAVING BEEN FORMED *under* THE GUIDANCE AND POWER of the our laboratories, formic acid, urea, &c. We see, therefore, that this MYSTERIOUS VITAL PRINCIPLE CAN BE REPLACED BY THE CHEMICAL FORCES"!!—Liebig's *Organic Chemistry applied to Physiology,* &c.—[See page 22d, second paragraph.]

"The INFLUENCE of poisons and of remedial agents on the living animal body evidently shows that the CHEMICAL decompositions and combinations in the body, WHICH MANIFEST THEMSELVES IN THE PHENOMENA OF VITALITY, may be increased in intensity by CHEMICAL FORCES of an analogous character, and retarded or PUT AN END TO by THOSE of opposite character; and that we are enabled to exercise an influence on every part of an organ by means of substances possessing a well defined CHEMICAL ACTION."—Liebig's *Animal Chemistry.* [See 3d motto.]

"With respect to the ACTION of quinine, or the alkaloids of opium, &c., physiologists and pathologists entertain no doubt that it is exerted chiefly on the brain and nerves. If we reflect that this action is exerted by substances which are material, tangible and ponderable; that they disappear in the organism; that a double dose acts more powerfully than a single one; that, after a time, a fresh dose must be given if we wish to produce the action a second time; all *these considerations,* viewed *chemically,* [*!*] permit only one form of explanation; the supposition, namely, that these compounds, by means of their elements, take a share *in the* FORMATION *of new* or the TRANSFORMATION of EXISTING BRAIN AND NERVOUS MATTER"!!—Liebig's *Animal Chemistry.*

"It is impossible to mistake the *modus operandi* of putrefied sausages, or muscle, urine, cheese, cerebral substance, and other matters, in a state of *putrefaction.*" "It is obvious that they COMMUNICATE THEIR OWN STATE OF PUTREFACTION TO THE SOUND BLOOD, from which they were produced, *exactly in the same manner as gluten* in a state of decay or putrefaction *causes a similar transformation in a solution of sugar*"*!*

"The MODE OF ACTION of a *morbid virus* exhibits such a STRONG SIMILARITY TO THE ACTION OF YEAST upon liquids containing sugar and gluten, that the two processes have been long since *compared* to one another, although merely for the purpose of *illustration.* [They have often been represented as *identical.*] But, when the phenomena attending the action of each respectively are considered more closely, VITAL PRINCIPLE, *it is this principle* which *determines* the direction of their molecular attraction."—"*The vital principle* ALONE is capable of restoring the original order and manner of the molecular arrangement in the smallest particles of albumen."—Liebig's *Lectures* for 1844.

"From the theory of disease developed in the preceding pages, it follows, obviously, that a *diseased* condition once established, in any part of the body, CANNOT be made to disappear by the CHEMICAL ACTION of a remedy."—Liebig's *Animal Chemistry.*

"THE VITAL FORCE IS SUBJECT TO THE ACTION OF A BLISTER."—*Ibid.*

"The VITAL FORCE in a living animal tissue appears as A CAUSE of growth in the mass, and of RESISTANCE to those external agencies which tend TO ALTER the form, structure, and COMPOSITION of the SUBSTANCE of the tissue in which the *vital energy resides.*"—Liebig's *Animal Chemistry.* [See 16th motto, and 4th paragraph, p.10, and 5th line p.12, right hand.]

The following unscrupulous declaration apappears to be addressed to that class of readers to whom I have adverted in the 3rd paragraph, p. 8. Thus:

"It is not the true chemist who has endeavored to apply to the animal organism his notions derived from purely chemical processes. He has not had the remotest intention of undertaking the explanation of any really vital phenomenon, upon chemical principles. The only part which chemistry now, or for the future, can take in the explanation of the vital processes, is limited to a more precise designation of the phenomena, and to the task of controlling the correctness of inferences, and ensuring the accuracy of all observations by number and weight. Although the chemist is able to analyze organic bodies, and tell us their ultimate elements, he does not claim the power of synthesis, or of producing them again by the union of these elements"!!!—Liebig's Lectures for 1844.—[See, particularly, 3d paragraph, p.10, 6th paragraph, p.14, left column, and 3rd and 15th mottos. Also, the opposite paragraphs.]

"The VIVIFYING agency of the BLOOD must ever continue to be the most important condition in the *restoration* of a disturbed EQUILIBRIUM, and the blood must, therefore, be considered and constantly kept in view, as the ultimate and MOST POWERFUL CAUSE OF LASTING VITAL RESISTANCE, as well in the DISEASED as in the UNAFFECTED parts of the body."—Liebig's *Animal Chemistry.*

Nevertheless,—

"No other component part of the organism *can be compared* to the BLOOD, in respect of the FEEBLE RESISTANCE which it offers to exterior influences."—"The chemical force and the vital principle hold each other in such perfect EQUILIBRIUM, that every disturbance, *however trifling,* or from WHATEVER CAUSE it may proceed, it will in REALITY be seen that their influence DEPENDS UPON THE SAME CAUSE." "*Ordinary yeast* and the *virus of human smallpox*, effect a violent tumultuous transformation, the former in vegetable juices, the latter *in the blood*"! "The action of the *virus of cowpox* is analogous to that of *low yeast* [!] It communicates *its own state of decomposition* to A MATTER in the blood, and from a SECOND MATTER IS itself regenerated"! "The *susceptibility* of infection by the virus of human smallpox MUST CEASE *after vaccination*, FOR THE SUBSTANCE to the presence of which this susceptibility is owing HAS BEEN REMOVED from the body by a peculiar process of DECOMPOSITION artificially excited"! "COLD MEAT is always in a state of decomposition. It is possible that THIS STATE may be communicated TO THE SYSTEM of a FEEBLE individual, and may be one of the SOURCES OF CONSUMPTION"!!—LIEBIG'S *Organic Chemistry applied to Physiology*, &c.

"IN ALL CHRONIC DISEASES, DEATH IS PRODUCED BY THE SAME CAUSE, NAMELY, THE CHEMICAL ACTION OF THE ATMOSPHERE." And so, also, of death from acute diseases; breathing, in all the cases, being the cause of death.—LIEBIG'S *Animal Chemistry*.

EFFECTS A CHANGE IN THE BLOOD."—LIEBIG'S *Organic Chemistry applied*, &c.

But, again, nevertheless,—

"It is obvious, moreover, that in all diseases where the formation of contagious matter and of exanthemata is accompanied by fever, TWO diseased conditions *simultaneously* exist, and two processes are simultaneously completed; and that the BLOOD, as it were, BY RE-ACTION, that is, fever, BECOMES A MEANS OF CURE."—LIEBIG'S *Animal Chemistry*.

"We can have no very high idea of experiments made by gentlemen, (*chemists*, with reference to digestion,) who, for want of anatomical knowledge, have not been able to pursue their reasoning even beyond the simple experiment itself."—JOHN HUNTER'S *Observations on Digestion*.

It only remains now to say of the foregoing contradictions (hitherto unnoticed,) that, whenever and wherever they, or their resemblances, occur in any department of science, we may of course rely upon it that the writer is in pursuit of any thing but truth, or is most inexcusably ignorant of his subject. In medicine, too, we may readily suspect that every thing is spurious which comes to us upon the wings of the imagination, or where we meet with tropes and metaphors instead of crucibles and acids, or with harmonious words instead of obstinate facts and hard thinking.

It follows, therefore, from what I have stated of the incongruities of the chemical school on the philosophy of digestion, as well as of all other functions of life, that I need not say much of the groundless nature of that mingled system of contrarieties by which the chemico-physiologists interpret the same functions. Their doctrine of digestion is thus laid down by the distinguished chemist, Dr. Prout; for you should understand that chemists, not physiologists and physicians, not the students of organic nature, are the great leaders in the medical schools of pure chemistry and of chemico-physiology. "First," says Dr. Prout, "the stomach has the power of dissolving alimentary substances, or, at least, of bringing them to a semi-fluid state. This operation seems to be altogether chemical.

"2d. The stomach has, within certain limits, the power of changing into one another the simple alimentary principles," and "this part of the operation of the stomach appears, like the reducing process, to be chemical; but not so easy of accomplishment. It may be termed the converting operation of the stomach.

⁂ The quotations from "Liebig's Organic Chemistry applied to Physiology," are derived from Mr. Playfair's edition, London, 1840; those from "Liebig's Animal Chemistry," are taken from Professor Gregory's edition, reprinted New York, 1842. The italics and capitals are mine.

"3d. The stomach must have, within certain limits, the power of *organizing and vitalizing* the different alimentary substances." "It is impossible," he says, "to imagine that this organizing agency of the stomach can be chemical. The agency is *vital*, and its nature completely unknown."

Such, then, is the doctrine of digestion as entertained by the chemico-physiologists. But, from what we have already seen of the absolute contradictions which abound in the writings of those who attempt the application of pure chemistry to the functions and results of organic life, we may expect that the chemico-physiologist will be equally inconsistent when he applies himself, at one time, to the phenomena of living beings, and, at another, reasons from the results of the laboratory to those phenomena. Accordingly, we find within a few pages of the foregoing doctrine of the chemico-physiologist, that he broadly affirms that "there is *no relation whatever* between the *mechanical* arrangements and the chemical properties to which they administer." "There is *no reason* why the chemical changes of organization should result from the mechanical arrangements by which they are accomplished, neither is there the *slightest* reason, why the mechanical arrangements in the formation of organized beings should lead to the chemical changes of which they are the instruments."

Here, then, in a single sentence, are not only the strangest contradictions, but a full admission that there is not the "slightest reason" for the application of chemistry to any process, function, or result of living beings.

Nor is this all. For the chemico-physiologist, the same eminent chemist whom I have just quoted, goes on to say, that "with the living, the animative properties of organic bodies, chemistry has not the *smallest alliance;* and probably will never, in any degree, elucidate those properties. The phenomena of life are not," he says, "*even remotely* analogous to anything we know in chemistry as exhibited among inorganic agents." And, as if to complete the overthrow of the chemical part of the philosophy of digestion, the same reasoner observes that, "The means by which the peculiarities of composition and structure are produced, which is so remarkable in all organic substances, like the *results* themselves, are *quite peculiar*, and bear little or *no resemblance* to any artificial process of chemistry;" that "nature will not permit the chemist to officiate as her journeyman in the most trifling degree." So, also, Dr. Roget, alike distinguished in the school of chemico-physiology. "Vital chemistry," he says, "is too subtle a power for human science to detect, or for human art to imitate." And to the same effect may be quoted Dr. Carpenter, one of the foremost in the school of pure chemistry. "The agency of VITALITY," says this reasoner, who generally ridicules the term and all that is supposed to relate to it, "*the agency of vitality*," he says, "as Dr. Prout justly remarks, does not change the properties of the elements, but simply *combines the elements* in modes *which we cannot imitate*." And thus might I go on with one after another, till I should have exhausted the whole that have attempted to confound the science of life with the science of chemistry, and show you by their own statements that there is not the slightest intelligible connection between them. Indeed, I have already, on a former occasion, pointed out this universal admission.

The ground of chemistry being thus virtually abandoned to the vitalist, it would seem superfluous to pursue an adversary who is always upon the retreat. But, as he flies, he is for ever shooting from behind, and his Parthian weapons fall thickly and heavily upon the vast multitude. He must therefore be subdued into a practical acquiescence with those consistent principles of nature which exact his consent, but not his compliance.

The subject of digestion supplies one of the numerous opportunities of pushing the chemico-physiologist upon open ground, and of exhibiting truth in its unadulterated purity. It is here, too, especially, that chemistry has reared its batteries, and whence it sends forth its artillery into the various dominions of the organic world. Yet it is here, that vitality is exemplified in its most impressive and astonishing aspects, and where unequivocal demonstrations abound that fluids, as well as solids, are endowed with the principle of vital operations.—It is here, especially, that nature has illustrated her distinction between the animate and inanimate world, and established her chain of connection. It is here, in the incipient change of dead into living matter, that we witness a full display of those powers which operate in the most elaborate organization, and an equal exclusion of the forces which appertain to dead matter. It is here the line of separation begins abruptly ; but where analogies are preserved in the conversion of dead into living matter, through new modes of combining the same elements ; and doubtless our admiration would increase, should we mount along the entire functions of assimilation, and find, at each step of the ascending series, that the whole agency has been committed to forces that have no existence in the inorganic world ; that the whole is the harmonious result of a principle which may form an intermediate link between spirit and matter ; and that there is no power within our control by which we can determine the nature of the changes. Casting a glance at the vegetable world, we find the connection continued, by other analogous links, with elementary matter itself; but here, as in the higher department of nature, the line of separation is equally defined, however low in the scale of analogy may be the properties of life which have their beginning in vegetable organization.

It is here, then, at the threshold of life, as in the propagation of the species, that we especially witness a substitution for Creative Power ; and as all that appertains exclusively to the organic world was perfectly distinct in its Creation from the inorganic, so are the substituted processes of generation, and of the conversion of dead into living matter, equally distinct from the causes and results of inorganic processes.

However, also, we may scrutinize the process of digestion in either of the organic kingdoms, we find that nature has provided intricate means for the purpose, and established analogies in their organization as well as their phenomena, that are unknown in the inorganic world. And if we carry the analysis through the whole labyrinth of animal and vegetable creation, we meet, at every step, with so much Unity of Design, such gradual modifications of structure and functions, and all conspiring to a specific end, we must conclude that the whole are governed by forces peculiar to themselves.

But, notwithstanding the astonishing Unity of Design, and the gradation of analogies which connect together the most dissimilar of the systems which are designed for digestion, there are peculiarities attending each in every species of animals, which place at a remote distance the laws of chemistry. "An alimentary canal is observed in every class of animals, and almost in every species, but its form and structure vary according to the situation of animals in the scale, or according to the kind of food on which they are destined to subsist, and the extent of the elaboration it requires to undergo. The peculiarities presented by the digestive organs are, therefore, intimately connected with the diversities of form manifested by the organs of animal life, and with all the living habits and instincts of animals." We see, also, an extension of this principle in relation to certain periods of life. In ruminating animals, for instance, the fourth stomach is alone employed during dentition. And yet, with all this variety, there is but one common result, one homogeneous chyle. The same principles, the same vital impulses which ultimately develope and call into action the three first stomachs of the ruminantia, appear to be not less concerned in the consummation of their final cause.

One of the most important arguments in favor of vital digestion consists in the remarkable *endowments* of the stomach, as manifested by its vital signs, and by the sympathies which prevail between this organ and all other parts. The final cause of this peculiar constitution of the stomach, this lavish supply of the properties of life, this subservience of other organs to its dominion, must be sought in its adaptation to the generation of a fluid that may bestow the first and most difficult act of vitalization upon dead matter. There would also have been something harsh and abrupt in nature, to have admitted into the recesses of her living organization mere dead matter. It is opposed to all analogy, and is, therefore, opposed to all reason. But, that a fluid should perform this astonishing office, this first and great step in the ascending series, it must possess in a high degree the principle of life. Mysterious as it may be represented, we must all of us come at last to the admission of the existence of a vital principle; yet far less mysterious, and far less difficult of comprehension than the human soul. It is fair, then, to conclude that an organ destined for such a high function should possess that principle, in common with all other parts, as the means on which its function depends; and the best evidences in favor of this analogical inference are to be seen in its diversified manifestations of life. To deny the dependence of vital results upon specific powers, necessarily involves an exclusion of all the evidence by which we infer the existence of gravitation, or of the intangible properties of matter.—As well may we doubt the reality of spirit, "or the Maker of the eye, because it cannot see Him that made it."

We have seen, also, that it is conceded by philosophers who defend, *in extenso*, the chemical hypothesis of life, that there may be *something* appertaining to the stomach totally distinct from the chemical powers, and which is capable of imbuing the chyme with *vitality* and an *organic* condition; and it is, therefore, quite a philosophical conclusion, that this *vital something* has an important agency in preparing the material for the admitted exercise upon it of the vivifying or organizing power. Nor can there be any valid objection to the supposition that this vitalizing power, which so far transcends the chemical forces in the organizing effect it is allowed to exert, may be fully adequate to any transmutations the food may undergo; and this inference is the more corroborated by the consideration that matter already in an organic state must be better fitted for the process of vivification, than it can possibly be after its elements are broken up and recombined by forces with which those of life are in absolute opposition. Besides, the vitality of the gastric juice, or the vital influence of the stomach itself, being fully admitted, and even capable of organizing the food anew, this, in itself, should protect the alimentary matter against any chemical agencies which have been supposed to operate. That this counteracting power, indeed, prevails to the full extent which I have alleged, appears to be rendered certain by the ordinary absence of any of those chemical changes which take place where numerous substances are mixed together out of the stomach,—substances which often possess strong chemical affinities for each other, and whose operation within the stomach would be promoted by its high temperature. On the contrary, whatever the variety, it is uniformly resolved into one and the same homogeneous substance, utterly unlike the results of chemical reactions of one kind of food upon other kinds; and what is also as conclusive as it is astonishing, the chyle is apparently the same substance in all animals. Chemistry must here be consistent with itself, and not renounce, for the sake of hypothesis, those precise laws by which, in its legitimate pursuit, it lays open, with astonishing exactness, what had appeared the arcana of nature. Here, too, upon the chemico-physiological hypothesis, is presented an instance in which it must be necessarily assumed that the properties of life and the forces of chemistry must act together in concert in converting dead into living matter, —one destroying, and at the same moment the other vitalizing; whilst the assumption is contradicted by all that is known of the relation of these forces to each other.

It will be also readily conceded that it is the demand of philosophy not to multiply causes, where one is perfectly adequate; and especially where it is admitted that all the others are of themselves wholly inadequate.

This remark may be also equally applied to a common assumption which is set forth in the following apparently plausible manner. "The vitalists," says one of their opponents, "are loth to admit the operation of chemical agents at all, and would seem to consider it derogatory to suppose that any changes, save the subtle ones effected by the powers of life, are worked upon the aliment." "The *vital principle*," he says, "whatever it may be, incessantly makes use of chemical and mechanical agents for its purposes; and it is no more degrading to it to employ an acid liquid, and a triturating process, in order to digest the aliment, than it was to have recourse to bony levers, cartilaginous pulleys, and tendonous ropes."

Here, in the first place, you will observe an entire begging of the question as to digestion by an acid, since that has never been shown, and is the main point at issue. It is a perfectly unfounded and extorted inference from the factitious analogy supposed to be seen in the admitted mechanical movement of the food in the stomach, bony levers, cartilaginous pulleys, &c. But the pretended analogy, I say, is utterly inapplicable, were it admissible to reason from better premises of this nature to the existence of important facts which have no other foundation. The bony levers, muscles, tendons, heart, and large blood-vessels, are mere instruments acted upon by the vital principle, and have no part in the vital results, except as they are the passive instruments of the properties of life. You will observe the same distinction between the process of digestion, and the mechanical movement of the food in the stomach, or the "trituration" of the food, as it is erroneously called by the writer just quoted; since food is not triturated by the stomach excepting where that organ is designed to supply the place of teeth. You will observe, I say, the total want of analogy between that mechanical movement of the food, and the assumed action of an acid; since, in the latter case, a radical change is supposed to be wrought in the alimentary mass, whilst no such change is wrought by the mere movement, or even by the trituration or grinding of food in the stomach. The contractions of the stomach, which are purely of a vital nature, facilitate the process of digestion; but they do no more than to expose the food freely to the action of the gastric juice, by which, alone, the conversion into chyme is performed. The contractions, or "trituration," are exactly on a par, as auxiliaries to digestion, with the teeth, or with the knife, which divide the food. The *acid* alone applies to the supposed chemical process of chymification. This is the only agent, involving the only force distinct from the vital principle that is supposed to operate, and to take part with the properties of life in the functions which belong to those properties. Nor is this all. Those chemical forces, or an equivalent agent, are supposed to appertain to the gastric juice, (a product of the most highly endowed organ in the animal system); and through that product, and by that product, to operate independently of the vital properties, or, under their control. But, here it may be again affirmed that throughout nature there is not an analogical fact to warrant the conclusion; and with equal truth it may be said that there is nothing to aid our conception of the *cöoperation* of the chemical and vital forces, whilst all that is known of their relations to each other proclaims their absolute independence.

But, again, it is the admitted final cause of the gastric juice to bestow life upon dead matter, whilst it is incontrovertible that inorganic matter is insusceptible of any such influence from gastric action.—Every fact proclaims that nature has provided the *vegetable kingdom* for the purpose, especially, of determining organic combinations out of inorganic substances for the sustenance of animal life. In the language of Liebig, "The first substance capable of affording nutriment to animals is the LAST product of the CREATIVE ENERGY,"—ay, "the CREATIVE ENERGY," he says, "of vegetables."—(Animal Chemistry.) It is manifest, therefore, that it would be an absurdity on the part of nature to have ordained that chemical agencies should operate even at the very threshold of life, at the very fountain for which she had provided elaborate means to subvert the combinations of chemistry, and to bring them into those entirely new arrangements that approximate the changes they are destined to undergo in the animal stomach. And far less probable is it, that this fundamental principle should be lost as we ascend from vegetable to animal organization; since every chemical result within the stomach would tend to reduce the aliment to the state of that inorganic matter whose complete reduction into organic compounds was effected by the vegetable kingdom for the uses of the animal. Such chemical results, therefore, would counteract the great final cause of nature, in either organic kingdom; and, in the animal, would render the means of sustenance more and more indigestible, and progressively liable to the condition of inorganic matter. This is fully allowed by the chief of the school of pure chemistry; as shown in the foregoing parallel quotations.

But again I say, if the vital principle be "capable of making use of chemical agents," no reason can be assigned why it may not be equal to the whole work of digestion, and of every other process of living beings. The *simple* construction may be comprehended, whilst the other is utterly unintelligible. The former alone is agreeable to the rules of philosophy, and abolishes the inextricable confusion which attends the chemical hypothesis. What, indeed, can be meant, by the vital properties making use of chemical forces? Can there be a more glaring absurdity? more absolute nonsense? How are those chemical forces brought into use, how held in subjection, how for ever maintained in one exact operation in each particular organic process, of which there are multitudes, distinct from each other, going on in the same individual? How do they elaborate from one common, homogeneous fluid, either the blood, or the sap, all the various, unique, unchanging, secreted products of the whole organic being? Products, for ever the same in every part, yet differing from each other according to the nature of the part? Did you ever hear or dream of any thing analogous to this in that inorganic world where chemistry holds its empire?—When do those chemical forces begin to operate, in the living body, what part do they perform, and what is the allotment of the properties of life? Is there any *known* concert of action between the two species of forces? On the contrary, is it not every where demonstrated that the properties of life are in direct opposition to the forces of chemistry?

Whatever be the construction, by uniting the two forces, (as is done by the only chemical school that is entitled to a respectful notice,) we convert what is a simple problem, like all other processes of nature, into the greatest paradox that has been yet devised by the ingenuity of man. It is in vain to say that some one or two of the products of organization, such as carbonic acid, and urea, are such as result from chemical affinities, since these are excrementitious; whilst chemistry assures us that all organic compounds are utterly different in their elementary combinations from any compound of a chemical nature.

Thus might I go on to argue this subject upon general principles alone; whilst at every step of the argument, we should see the whole chemical hypothesis of life taking its proper rank as a dream of the imagination, or as a project of ambitious minds.

But, I must hasten to the specific facts which are brought by the chemical and the chemico-physiological schools to the support of their preconceived hypotheses; for you must understand that digestion was regarded as a chemical process, and according even to its present interpretation, whilst chemistry was yet in its infancy.

Digestion having been assumed to be more or less, or altogether, a chemical affair, it rationally followed that it might be imitated by art. Accordingly, when this ambitious science had succeeded in turning the whole inorganic world into the laboratory, it set itself at the manufacture of organic compounds, and even at the entire animal. It did not, like Alexander, sit down and weep because it had no more worlds to conquer; but, like Shakspeare, having "exhausted worlds, it then imagined new." Even eminent physiologists, who should look with jealousy upon any invasions upon the laws of nature, especially upon such as it is their peculiar province to illustrate, began the manufacture of gastric juice by fire and acids, and metallic salts. We are thus presented by these philosophers with artificial compounds, of a most incongruous nature, and we are told that each one is the gastric juice; that each is capable of the same precise results as that universal product of animals, apparently the same in all, and elaborated from the blood by an organ of the highest vital endowments, and to which there is nothing analogous in all the other products of living beings, each product being, also, equally unique, and all derived from one common source.

A diversity of opinions exists as to the real nature of the chemical agent supposed to be employed by nature in the process of digestion. Free muriatic acid having been found, or supposed to exist, in the stomach, it has been concluded by many that this must be the great agent; whilst Dr. Prout, and others, affirm that "free muriatic acid more or less retards the process of reduction." Dr. R. Thompson, however, states that, by digesting muscular fibre in dilute muriatic acid, he produced a substance "exactly resembling chyme." This experiment was pretty widely repeated, and many were equally successful with "dilute muriatic acid" as was Dr. Thompson. Others, on the contrary, declared their failure, and others, like Dr. Prout, maintained that this acid retarded digestion. Eberle had already advanced the hypothesis that mucous membranes, no matter whether of the stomach or the bladder, dissolved either in muriatic or acetic acid, would form the true gastric juice, and perform its wonderful operations. There is now a general bias in favor of one of these compounds, though other preparations are supposed by many to form very good gastric juice. Again, it is said that the "digestive mixture," as it has been well denominated by the manufacturers, "retains its solvent properties for months," whilst the gastric juice loses its solvent power soon after its abstraction from the stomach. And what equally establishes a total difference between the "mixture" and the gastric juice is the no small circumstance that the chemist may torture and extinguish the artificial "digestive principle" in a variety of ways, and then transmute it back in all its vigor. Thus, according to Schwann and Müller, the artificial "digestive principle" may be neutralized by an alkali, and afterwards "precipitated from its neutral solution by *acetate of lead*, and obtained again in an active state from that precipitate by means of hydrosulphuric acid." This precipitate, we are told, when thus treated, and thus compounded of principles radically different from the original mixture, is essentially the same as the gastric juice, and that the results of such artificial preparations must be taken as the test of the *physiology* of natural digestion; that, abandoning nature, we must look to the resources of the laboratory for any satisfactory account of her vital processes. Nor do I at all exaggerate; for it is distinctly avowed that we knew nothing of digestion till the invention of the artificial mixtures. Thus, it is said of Schwann by one so able and distinguished as Müller, that he, (Schwann) "having discovered that the infusion of dry mucous membrane with dilute acid, even after it is filtered, still retains its digestive power, the digestive principle, *therefore*, is clearly in solution, and *the theory of digestion by contact falls to the ground*." Here, a most important physiological induction is wholly founded upon a process which has not the most remote connection with organized matter; and as to the paradox which involves the combined agencies of muriatic acid, alkalies, acetate of lead, hydro-sulphuric acid, and mucous membranes, it is certainly a burlesque upon the science of chemistry.

I have said that the experimenters took the hint of manufacturing gastric juice from the occasional discovery of an acid in the stomach. But, this is undoubtedly a rare phenomenon in a healthy stomach, and where the food has been at all appropriate in quality and quantity.—The chemical hypothesis, as I have said, was long ago in vogue, and was put at rest by demonstrative proof. Distinguished observers, Hunter, Haller, Willis, Spallanzani, Fordyce, and more recently Dumas, Schultz, and others, insist that the reputed acid is the result of a true chemical decomposition of vegetable matter. Spallanzani, whose experiments were almost endless, Scopoli, Chevreuil, and others, rarely succeeded in finding it at all, and in some animals never. Spallanzani, indeed, affirms that the gastric juice is neither acid nor alkaline in its natural state.

As far back as Haller's day, when this subject was agitated, it is said by this illustrious and accurate observer, that, "although there may be some rare signs of an acid in the stomach, it does not, therefore, become us to suppose that food is animalized by a chemical process; much less to compare this process with the action of an acid." And, anticipating the modern experiments with the "digestive mixture," he declares of analogous proceedings at his own era, "frustra etiam quisquam, imitatus liquores acres chemicos, liquorem corrodentem invenerit, qui carnem in pultem resolvat." And there can be no doubt that Hunter's prophecy holds good to this day, that—

"If ever any matter is formed in any of the juices secreted in any part of a vegetable or animal body similar to what arises from fermentation, we may depend on it, it arose from that process; but we may also depend on it, that there is a defect of the living principle in these cases."

These are not the mere speculations of genius, but the facts and the conclusions of genius after a long, and wide, and experimental survey of nature.* And are these observations, nay, our own experience, our own senses, to be set aside to accommodate a hypothesis of life which identifies dead, even inorganic, with living beings?

I might thus go on to multiply proofs of the foregoing nature, not a little of which is supplied by distinguished observers of the present day; and a variety of other facts might be brought in opposition to the chemical hypothesis of digestion. But time presses, and I must come to a conclusion; but not without expressing the conviction that it is here, especially, that the physiologist must raise his principal defence against the invasions of chemistry. Defeated upon this ground, the adversary may be easily expelled from the whole domain of organic life.

* See mottos, 1st, 2nd, 3rd, 5th and 16th paragraphs. Yet is it said that the chemical speculations "must command the respect of their opponents."

⁎ Paine's *Medical and Physiological Commentaries*, in 3 volumes, may be had *only* at the Author's residence, in the City of New York. No. of royal octavo pages 1938. Price of the 1st and 2d volumes, $6.00. The 3d volume without charge.

A

DEFENCE

OF THE

MEDICAL PROFESSION

OF THE UNITED STATES;

BEING

A VALEDICTORY ADDRESS TO THE GRADUATING CLASS

AT THE

MEDICAL COMMENCEMENT OF THE UNIVERSITY OF NEW YORK,

DELIVERED, MARCH 11, 1846,

BY

MARTYN PAINE, A. M. M. D.,

Professor of the Institutes of Medicine and of Materia Medica in the University of New York;
Member of the Royal Verein für Heilkunde in Preussen; of the Medical Society of Leipsic;
of the Montreal Natural History Society, and other Learned Associations.

TENTH EDITION.

NEW YORK:

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1846.

The author of this Address has seen, with much surprise, in some of the periodicals, a misapprehension that the "Chairman of the Committee" is the object of his rebuke. The author, therefore, will point the attention of the reader to the obvious construction that the "Chairman" was merely employed as a weapon against the *New-York State Medical Society*, by whom his opinions were sustained.

It is also worthy of record, that Dr. Davis was made "Chairman" of the Committee, "appointed *to present the subject of medical education*," by the late abortive Convention held in this city, whilst Dr. Hays made the Report of the Committee's "Preamble and Resolutions."

PROFESSOR PAINE'S DEFENCE

OF THE

MEDICAL PROFESSION OF THE UNITED STATES.

GENTLEMEN, GRADUATES!

IT devolves upon me to congratulate you on the attainment of that distinguished honor which denotes a high order of intellectual culture, and thus to welcome you to the bosom of the Medical Profession.

And this, gentlemen, I do with greater cordiality, from my general acquaintance with you, and from my knowledge of your qualifications for the exalted functions of Doctors in Medicine.

You enter, my friends, upon your career, with the certainty of immediate usefulness; for although more extended observation of disease will impart greater confidence and skill, you have acquired a knowledge of those great principles in medicine which assemble the facts and the experience of ages, and which, especially, are to form your guide in the treatment of disease.

"I have done my part," says Sydenham, "by mentioning the indications to be considered, and pointing out the time, and manner of doing it; for the practice of physic consists chiefly in being able to discover the true curative indications, and not medicines to answer them; and they who have overlooked this point, have taught empyrics to imitate physicians." The illustrious Senac sets forth the true aspect of this subject in his work on Fever: "For the better illustration," he says, "of the method of cure, I had prepared for publication a number of cases. But, it appeared best, on reflection, that these should be omitted. There are not two cases of disease precisely alike; whence, there are not two in which the same remedies will produce precisely the same effects. It seems more advisable, therefore, to reduce, to certain general laws, all the facts which experience has brought to light, and from these, to deduce a mode of cure accommodated to the state and condition of each patient."

And such have been the conclusions of all the great apostles of medicine. Experience, and facts, in the first place; and then let us have them reduced to principles as substitutes for them. I speak, however, of that experience which Zimmerman says is "generally considered the simple produce of the senses. The understanding seems to come in for so small a share, that every thing intellectual in it is regarded as having as much materiality in it as the perceptions of the senses. This," he says, "is what I call false experience. It is a blind routine, directed by no law whatever,"—and hence, "the old man is generally supposed to have thought more than a young one, because he has seen more."

But, says the same eminent philosopher,—"it has been already remarked, long before my time, that the increased number of years and patients only serve to remove physicians, destitute of genius, farther from true physic. The more their practice is augmented, the more numerous and considerable are their errors. On the contrary, we observe, that, by the aid of sound judgment and knowledge, a physician is enabled to penetrate the greatest difficulties, *even from his youth*; and that with these to guide and support him, he rises superior to every obstacle." It was the opinion, indeed, of this accurate observer of man, that,—"he, who at thirty years of age is not an able minister, an able general, or an able physician, will never be so."

It is not, therefore, my young friends, the great number of undigested cases, but the comparatively few that are well observed, and well considered, that contribute, with the great principles of medicine, to form the enlightened and successful practitioner. Hippocrates, according to Galen, gathered his knowledge in small towns, insufficient to support a single physician. In this acception he was only a country practitioner. But, as Socrates said of another ancient, "he spent his time in inquiring, considering, and consulting;" and was thus enabled to lay a foundation which will forever procure for him the familiar appellations of "The Father of Medicine,"—"the senex divinus," the divine old man.

I have thus, gentlemen, stated to you the conclusions at which the most observing and enlightened physicians have arrived in respect to the early qualifications of the industrious student for the practice of medicine, that I may do you the justice to which I know you are so well entitled; that I may contribute to your immediate rank amongst the more advanced in professional experience; that I may stimulate the tardy confidence of society in the knowledge and ability of the young, but well educated graduate in medicine; and that they may know from mature age itself, from the long experience in which their confidence has grown into a sentiment of veneration, that gray hairs and a furrowed brow are comparatively insignificant testimonials of knowledge and skill, with the credentials of those who are allowed to be amongst the foremost in medical rank, who are allowed to cherish the science as the most sacred of all the gifts to man of the Great Wise and Beneficent Being.

This day, gentlemen, you have been officially transformed from the pupil into the teacher,—from the advised into the adviser,—and he who now addresses you, so late your monitor at the bed-side of the sick, is now on common ground with yourselves, may meet you to-morrow to deliberate upon the intricacies of disease, and is bound to yield the same respect to your opinions that he may desire for his own.

You have been, my young friends, sifted from the "chaff;" you have proved yourselves worthy the elevated post which you now occupy amongst the family of mankind; that you are capable of its responsible duties; and there is no act of my life which yields me higher gratification than that I am thus warranted in commending you to the confidence of your fellow men.

And now, that I have done all this, you must never forget that something is due to him, and to those, who offer this pledge of your professional merit,—never forget that it is in your behalf, and through their relations to the community, that this demonstration of confidence is made,—never betray your instructors, your advocates, your guarantees, by one act that shall degrade yourselves, or bring mortification to your early benefactors, or one stain upon your Heaven-born profession.

Assume, therefore, gentlemen, at once, a manly confidence in yourselves; go forth to improve the general condition of society, to minister relief to suffering humanity, to warm many a heart with the purest and noblest sentiment of gratitude, to gather fresh laurels for yourselves and for your profession, and you shall find that the practice of industry, and virtue, and benevolence, will unceasingly urge you on to a strife within your own bosoms for greater deeds of that unalloyed happiness which awaits the blessed in God.

But, although you be now qualified as good practitioners, you will become better and better as observation goes on, and as you study the experience and the wisdom of the past. As I said to your predecessors in my Introductory Lecture on the IMPROVEMENT OF MEDICAL EDUCATION IN THE UNITED STATES, "you must aspire at the goal of medical philosophers, before you may comprehend or enjoy, in its ample dimensions, the philosophy of medicine.

You must have willing minds, and steady purpose, before you will study the elaborate writings of its sages, and bring to their interpretation, and an exposition of their uses, the requisite habits of observation through the wide domain of Nature."

Nor can I do better, at this time, than to recite another passage from that Lecture on medical education.

I said to my audience, that, "when your professional harvest begins, then is the time for the most salutary stimulus of ambition; and, whoever yields to its spur, will find abundant opportunity to carry his knowledge to the highest stretch of his intellectual constitution. He has but to remember that professional rank can only be acquired by various learning; and in the language of Johnson, 'he, who proposes to grow eminent by learning, should carry in his mind at once the difficulty of excellence and the force of industry; and never forget that fame is not conferred but as the recompense of labor; and that labor, vigorously continued, has not often failed of its reward.' These indisputable precepts compel the aspirant at medical renown, or even at the highest practical success, to limit his enjoyments to a social intercourse with his patients, to a participation in the general enterprises of society, to the study of nature, and, lastly, to all the literature which may elevate the mind. These should be his recreations as well as his employment; and as knowledge flows in, an inexhaustible fountain of happiness is acquired, whose very spray clouds the gaieties of life, and renders the pleasures of the intellectual man as circumscribed as the voluptuousness of wealth is discursive in its search after sensual gratifications. One is a steady, refined, and unalloyed enjoyment; the other, gross, discontented, and forever sated with every thing but the new. Philosophical happiness, says Burke, 'is to want little; vulgar happiness is to want much, and to enjoy much.'

"A great work," I said, gentlemen, "is to be done by the physician. But industry, like love, will conquer all things. *Amor vincit omnia,'—Industria omnia vincit;* and in proportion as deficiencies abound when the physician assumes the high responsibilities of his profession, it becomes him to investigate well every step of his experience, to employ his hours, or his moments, of leisure, by day and by night, in garnering the experience which is supplied by the literature of the profession; and, by rendering experimental observation and theoretical principles mutually subservient, he shall come, by degrees, to the attainment of distinguished worth, and the applause of mankind."

Such, then, were some of the remarks which I addressed to novices in medicine, that they might be early led to consider the difficulties which laid before them. The same remarks are more appropriate to yourselves, who are about to assume the responsibilities of the profession, and to take an active part in promoting the advancement and the dignity of medicine.

And this leads me, gentlemen, to consider the objects of the approaching National Convention of Physicians in this city, and how far the interests of medicine are likely to be promoted by this contemplated event. The present occasion seems peculiarly appropriate for casting a broad glance over the existing state of medicine in this country, of considering its defects, and how they may be removed, and how, in other respects, our noble science may receive, at the hands of a National Convention, that impulse which must be ardently desired by every enlightened and honorable mind. But it is due to my colleagues to say, that in all which I shall set forth, I am alone responsible,—am alone aware of the subject which I am about to discuss.

The project of this National Convention of Physicians originated with a young man in the township of Bingampton.

It may be also proper that I should premise that he was subsequently elevated to the dignity of *Chairman of the Committee of the New York State Medical Society*, by whom his project had been espoused. To him has been delegated the work of expounding the contemplated objects of the Convention, and, unfortunately, to him the correspondence with the Medical Colleges.

But, gentlemen, it is now time that I should lay before you the Circular of the State Medical Society, concerning a general meeting of the profession.

That "Circular" sets forth,—that:—

Whereas, It is believed that a NATIONAL MEDICAL CONVENTION would be conducive to the *elevation* of the *standard* of *Medical Education* in the United States. And

" *Whereas*, There is no mode of accomplishing so desirable an object without *concert* of action on the part of the Medical Societies, Colleges, and Institutions of all the States. Therefore,

" *Resolved*, That the New York State Medical Society earnestly recommends a National Medical Convention of Delegates from Medical Societies and Colleges in the whole Union, to convene in the city of New York, *on the first Tuesday in May in the year* 1846, for the purpose of adopting some concerted action on the subject alluded to in the foregoing preamble.

" *Resolved*, That Doctors N. S. Davis, James McNaughton, and Peter Van Buren be a committee to carry into effect the above resolution.

PETER VAN BUREN, M. D.
Secretary of New-York State Medical Society.

[Dated,] ALBANY, February 6th, 1845."

When I first heard of this Circular, I had hoped much from amicable and dignified consultations amongst the Delegates that might be assembled from all parts of our Union ; but, finding, at a later period, that the Projector of the Convention and Chairman of the Committee was traducing the profession and the Medical Colleges through the medical periodical press, my hopes gave way to apprehensions that the objects of the Convention had not been conceived in the spirit which had been set forth by the Circular. I had already submitted to the profession my own views of the state of medical education in this country, and of the embarrassments which it would be required to encounter, till a greater diffusion of wealth should enable us to compete with the European States ; and I had hoped that some mitigating expedients might be honorably and amicably devised by which that large and poorer class of medical students might be so aided as to enable us to extend the term of instruction.

Nevertheless, I demonstrated then, as I had already done in the *Medical and Physiological Commentaries*, a great ascendency in the practical habits, and in the successful treatment of disease, by American over European physicians. The last exposition of these views was made in my Introductory Address on the "*Improvement of Medical Education in the United States*."

Having, therefore, in that Address, considered the existing defects of medical education in this country, and the difficulties it encountered, it would be unprofitable to travel again over the same ground. My convictions, as then expressed, have, through the light of farther experience, received the most entire confirmation.

It may not be superfluous, however, for your present information, should I recite a passage from that Lecture, which comprehends the fundamental principle that lies in our way to those early achievements in science which are more common in the wealthy countries of Europe. I said, gentlemen, in that Lecture as a deduction from my premises :—

" Thus, then, I destroy the parallel which has been attempted between our own and European States, and show it the merest fiction of the visionary mind. Exact from our physicians the intellectual culture, and rear in this land the high standard of medical acquirements which are so noble and fascinating in some of the schools of Europe, and quackery will reign almost universal from one end of the continent to the other. Is not the whole multitude, whether rich or poor, pressing forward either for greater wealth, or for the pittance of their daily bread ? Nay, more, do not all our Medical Colleges hold out the temptation of moderate fees, and give, in their annual announcements, a conspicuous place to the humble charges for the necessaries of life? But what are these compared with the expenses attendant on the prolonged and higher grades of academic and medical learning in some of the European States ? And who does not see the inconsistency that would hold in one hand professions of cheapness to allure the student through our present system of medical discipline, and threaten with the other augmented fees and an impossible exaction upon time? The same principle runs through all our primary schools, our academic, collegiate, legal, clerical and political institutions. Cheapness of education, and a corresponding adaptation of time, are found indispensable to the general condition of society.

"The question is therefore settled upon the immutable principles of truth—of a truth which is founded in the exigencies of our country. We have not the means, we have not the leisure, to follow the standard of European wealth; nor can we control our destinies by European legislation. He, who, in America, aims at the profession of medicine, with honors and dignities as inviting as in the aristocracies of Europe, but less seductive than the allurements of wealth, comes from a class where the blandishments of the latter have no existence. He has worked his way from elementary schools through the higher departments of academic learning, under the frigid discipline of poverty, and he enters our halls of medical education with little else than the hope that his career may not be arrested by insane exactions, now for the first time borrowed from the overgrown wealth of Europe, and her old and rich institutions.

"Raise, therefore, beyond a certain limited poise, our standard of absolute requirements, and I repeat it, with no fear of contradiction, we shall turn from our medical schools most of their aspirants into more humble channels, or into the walks of empiricism. The exigencies of American physicians demand an early application to the business of life. If we would cultivate the field of medicine, we must look for an early harvest, or, my word for it, it will be soon overrun with weeds. But these necessities by no means preclude the highest advancement in medical attainments."

So far the Lecture. But it seems proper that I should state that it has been objected to my broad principle, that there are professional men in the populous towns of Europe, who have risen to the highest eminence under the most severe pressure of want. And *so*, gentlemen, is there an average proportion of the same description in the United States. But these are examples in which *genius* displays its greatest attribute,—examples which have rendered it a familiar proverb, that "*genius struggles with poverty.*" They are examples, as the Chairman of the Committee says, of "*minds, so endowed by nature, that despite of all the contracting and evil influences of our system of education, will rise superior to every obstacle, and shaking off one embarrassment after another, will roam over, and revel in, all the fields of science untrammeled and free.*" But he is an imperfect observer of man, who carries the principle through the community, or supposes that one in a hundred may be induced, or compelled, to struggle with poverty for the sake of an "elevated standard of science." Gentlemen, it is a delusion, which admonishes us of the danger of rendering what is rare the general rule.

It remains now only to consider at this time, how far the objects of the Convention, *as contemplated by the State Medical Society*, are likely to be fulfilled under the auspices of that Society.

Nothing can be gathered, of a specific nature, from the general Circular, and we must therefore go for information to expositions set forth by the Projector of the Convention and Chairman of the Committee.

That he is duly authorized to make such Exposure appears not only from the Circular itself, but from an article from his pen in the November number of the *New York Journal of Medicine and the Collateral Sciences*, in which he says, that:—

"There is, at present, a fair prospect for a *full* and *animated* Convention. This being the case, we, (the Committee,) hope it will not be deemed out of place, for us to write and the medical press to publish, a few thoughts on the PRESENT CONDITION and wants of the profession in relation to the education of its members, that the appropriate remedies may be the more clear and satisfactory."*

That, also, I may not be supposed to have mistaken the origin of this National Convention, and as it may be useful to be assured of the fact, I will quote a passage from the January number of the same *New York Journal of Medicine*. Thus:—

"*We invite*," says the editor, "*particular attention to the following very just remarks from the gentleman who originated the plan of a Convention, as recommended by the State Medical Society*." The article which then follows is from the Chairman of the Committee. Besides, we have it from the Chairman himself, in a letter to this Faculty, that such is his title to that distinguished conception; although long before the subject of discussion by the profession at large. In that letter he says, what cannot be mistaken, that, "*the proposition for a National Convention was made by myself*."

And having now thus settled this preliminary fact, and as all agree that he is *the Chairman*, and directs, from Bingampton, the movement of the 40,000, let us come, at once, to the specific objects of the State Medical Society, as expounded by the Projector of the Convention and Chairman of the Committee.

This we may learn, in the first place, from the article to which I have just referred, in the *New York Journal of Medicine.*

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* The Capitals and Italics are often mine.

Thus, then, the Projector of the Convention ;—"In regard to remedies for the present defects in our system of education," he says, "first of all, and at the foundation of all, we *place more care* on the part of individual practitioners concerning the *capacity* and *qualifications* of young men whom they receive into their offices as students; and then *more attention* to their education after they are received."

Now, that such evils exist, I concur with the Chairman; and I also agree as to their becoming appropriate subjects for united deliberation. But, I differ with him as to their magnitude and universality, and the terms in which they are expressed. Thus, he goes on :—

"No one can have failed to notice the almost TOTAL NEGLECT *every where* apparent in this respect. Whoever takes *a notion* to study medicine; no matter if his *primary education* (I wish you to remark, gentlemen, his *primary education*)—*no matter* if his primary education should not enable him *to write his own name intelligibly*, much less to understand a single branch of Natural Science, and his intellectual faculties naturally as dull as the *sleepiest drone* in the country; he is *readily received* into some office, shown a scantily filled case of Medical Text-books, and left *to while away the time* required by law before he can be honored with the title of Doctor. This is *a great and all pervading evil;* and it is in vain to exert ourselves to elevate the standard of education, until it is removed. *For so long as the mill is supplied with bad grain, it will grind out bad flour.*"

Here I arrest my quotation, for the present, that you may enjoy a repetition of its closing metaphor, varied a little in the elegance of its phraseology, and as submitted in a letter to the Medical Department of this University.

"If," says the Chairman, "we put *bad grain* into the *hopper*, (meaning you, gentlemen,) we expect *bad flour* to come through the *bolt.*"

Perhaps, gentlemen, we have now seen enough of the imputed prevalence of the radical evil; perhaps you have heard enough to abandon so unworthy, such a degraded profession,—scarcely advanced beyond that of Chili, of which Zimmerman says its members "*blow* about their patients, and think that they know enough when they know *how to blow.*" But I will venture to define yet farther the views of the Projector as to the extent of this delinquency, that you may the better comprehend who they are that are to meet together in convention and castigate each other into something better than merely *blowing* about their patients, and teaching each other how to *blow.* Here it is, then, fresh and pungent, from the Chairman himself. In the November number of the *New York Journal* already qnoted, the Projector says:—"There may, indeed, be found *here and there*, a mind, so endowed by nature, that despite of all the contracting and evil influences of such a system, will rise superior to every obstacle, *and shaking off one embarrassment after another, will roam over, and revel in, all the fields of science untrammeled and free.* And it is such that constitute the ornament and honor of our profession.

"*But far otherwise is it with the great mass;* THE NINETY NINE OUT OF EVERY HUNDRED. *With no practical knowledge of chemistry and botany; with but a smattering of anatomy and physiology, hastily caught during a sixteen weeks' attendance on the anatomical theatre of a Medical College; with still less of real pathology; they enter the profession, having mastered just enough of the details of practice to give them the requisite* SELF-ASSURANCE *for commanding the confidence of the public; but without either an adequate fund of knowledge, or that degree of mental discipline, and habits of patient study, which will enable them ever to supply their defects. Hence they* PLOD ON THROUGH LIFE, *with a fixed routine of practice, consisting of calomel, antimony, opium, and the lancet, almost as empyrically applied as is cayenne pepper, lobelia, and steam, by another class of men. And how can this ever be otherwise, while medical students, without regard to their previous qualifications, are shut up three years in the office of a country or village physician, sent out to one or two courses of lectures, and then honored with the title of M. D.?*" "INDEED SO RADICAL ARE THE DEFECTS *in our present system of medical education, and so glaring are their effects on the condition and usefulness of the profession, that no observing man will charge us* WITH EXAGERATION *in the foregoing statements.*"

Such, then, is the PORTRAIT of the medical profession in the United States, as *deliberately* drawn by the official hand of the Projector of the National Convention and Chairman of the Committee of the New York State Medical Society.

I mean, gentlemen, the portrait of "*ninety-nine out of every hundred*" of the medical profession in this country, or 39,600 out of 40,000; which reduces the number of worthy members to 400. It may be also proper to say, that the articles from which I shall have quoted were before the community prior to the meeting of the State Medical Society in February last, and that the same aspersions of the profession and of the Medical Colleges were then made by the Chairman, and by others of the Society.

Now, gentlemen, the interesting question comes up, as to the composition of the approaching Medical Convention. You, and I, have too much modesty to believe that we can be allowed to form a *fragment* of that small number of whom the Chairman says, they "can rise superior to every obstacle, and roam over, and revel in, all the fields of science untrammeled and free."—*We* must, in all humility, go with the "great mass,"—with the 39,600; and it is now, therefore, for us to decide whether we will meet in convention and *thus endorse the calumny*, or give it up to the *hundredth* man who is said by the Chairman to be alone competent to the work of reformation?

But perhaps there may be some Medical Colleges who may not fully comprehend the rank which they are assigned amongst the 40,000; and I will, therefore, through you, call *their* attention to the subject, that they may come hither the more understandingly.

The Chairman employs the device of contrasting the state of the profession in this country when we had scarcely a medical college, with its degraded state under the auspices of our thirty medical schools. Thus:—

"Not a few," he says, in the Journal aforesaid, "educated at that period, with their heads silvered over with age, possess a degree of mental discipline and sound learning, which should put to the blush THOUSANDS *of the graduates who now* YEARLY *emanate from our numerous well-endowed schools of medicine.*"

We have often heard much of this clamor about the "*thousands*" who are annually let loose from the Medical Colleges, to carry havoc into the chambers of the sick.

But, in sober truth, probably not more than *one thousand* receive medical degrees annually, from the whole thirty schools together; and a large proportion of these obtain their diplomas from such men as the Jacksons, Hare, Caldwell, Dunglison, Ware, Bigelow, Silliman, Horner, Nathan Smith, Gallup, Chapman, Dudley, Gross, Channing, Meigs, Mutter, Haywood, Drake, Yandell, Warren, Aikin, Bartlett, Gibson, Muzzey, Hodge, Harrison, and some others of not inferior note, whom I have thus hastily designated as controlling the "hopper" to which most of the "chaff" is sent, but who, I take it, the Chairman will feel obliged to concede belong to the 400 who are worthy a place in the American Medical Profession. To whom else, gentlemen, can he advert, if he exclude from the four hundred those men who sit in judgment upon the qualifications of most of the medical graduates in the United States?

These are they, gentlemen, who are denounced as sending forth "*annually thousands*" to inflict the mischief of abject ignorance upon a helpless community, and then *are asked to come here and sanction the ignominious reproach.* Why, gentlemen, what inconsistency. The Chairman cannot hesitate to allow that abler or more honest men cannot be found in the ranks of the profession; and yet he says that these very men dispense their approbation, the highest honors of medicine, upon ignorance and stupidity. Nevertheless, gentlemen, they are, what the State Medical Society, through the Chairman of their Committee, must admit to be amongst the best ornaments of the profession in this country;—aye, gentlemen, without superiors in the best of European Seminaries,—the *Dii immortales* of our profession.

In the hands of such men, our profession, our country, are safe! But what would be the destinies of that profession, the honor of that country, were they to fall into the hands of those who can see nothing in "*ninety nine out of every hundred*" of their professional brethren but abject ignorance and its cruel dispensations?

Since, therefore, it must be conceded, on all hands, that the professors in the principal medical schools of this country are eminently able and honest, do you not believe that they would sooner surrender their lofty and responsible trust than allow, according to the allegations of the Chairman, so much "chaff" to slip through the "hopper?"

We can, therefore, only hope that the profession will continue to supply the colleges with the same "sort of chaff," till better can be produced, and we will engage to turn out a quality of "flour" that shall pass for the best in any European market; quite as good, at least, as any that is manufactured by the *Censors* of State or County Medical Societies; and perhaps, therefore, it might be well for the country if a little more "grist" were brought to the "hopper." Perhaps it would not be amiss if even the "thousands," so appalling to the State Medical Society, were annually turned out of the collegiate "hopper."

And after all, gentlemen, may it not be, is it not probable, that the choice few, the 400 out of the 40,000, those scattered "ornaments of the profession who roam over, and revel in, all the fields of science, untrammeled and free," are quite as likely to have been "bolted" at the collegiate mill, as in the "green room" of the "holier" Censor?

And this leads me, reluctantly, to notice the remarkable fact, that amidst all the outcry about Medical Colleges, not a word has been ever alledged against those numerous "hoppers" which exist in the shape of "Censors" of State and County Medical Societies.

Not a word, gentlemen ; for that would be the "OUTS" against themselves,—against the aspirations of a noisy few, and against that plentiful "toll" which is deducted from every "licentiate" before *he* is allowed to run through the *accommodation* "hopper."

And now, perhaps, we shall have no difficulty in understanding why it is so earnestly desired to extend the term of instruction in our Medical Colleges, and also as a preliminary requisite to admission into these institutions. There is an *aristocratic* feature in this movement of the worst omen, however the spirit by which it is prompted may belong to the Agrarian policy. It is oppression towards the poor, for the sake of crippling the principal Medical Colleges. You, and all of us know, as I have already said, that the "great mass" of professional men in this country, Lawyers, Divines, and Physicians, belong to the poorer class of society ; and should the wire-pullers effect an extended term of education, it must exclude a large proportion of the "great mass" from the advantages of collegiate instruction ; whilst the aristocratic feature will be rendered still more offensive by limiting the attendants at Medical Colleges to the few who may spring from families of wealth. If such be the principles of republicans, if such the basis of our institutions of learning, their founders were less conscious of their objects than they who would degrade the poor by oppressive exactions.

I shall not, gentlemen, discuss this small affair any farther with the State Medical Society, who speak to us through their organ, the Chairman of their Committee ; but I will read to you a paragraph which occurs in the same number of the Journal of Medicine, from which I have made a quotation from one of the Chairman's animadversions upon his profession, and which has the merit of following, almost immediately, the article contributed by the Chairman. It is an extract from Percival's *Medical Ethics ;* and, gentlemen, whether you remain members of the profession, or seek other employment, I trust the sentiment will be forever the foremost amongst your hallowed obligations. Here it is, brief, as it is sacred :—"Every man," says Percival, "who enters into a fraternity engages, by tacit compact, not only to submit to the laws, but to promote the *honor* and the interest of the association, so far as they are consistent with morality, and the general good of mankind. *A physician, therefore, should cautiously guard against whatever may injure the general respectability of his profession ; and should avoid all contumelious representations of the faculty.*"

I say, gentlemen, that I trust,—I have no doubt, that that sentiment has become as intimately incorporated with your very nature as is your soul with your organic fabric.

I said, gentlemen, that I did not intend to discuss this subject any farther with the State Medical Society. But, *as a member* of that "great mass" of the profession, the "ninety nine out of every hundred," who have been presented to the world as a rotten excrescence, I shall state to you what had been my own views of its condition *before I was elevated to a professorial chair*, and which it is now my happiness to confirm by greater observation, and by the contribution which the Medical Department of the University has this day made to the general weal. This is due, at least, to myself as one of the 'ninety-nine" designated by the Chairman, and I trust will not be ungraciously received by my companions in ignominy. In the *Medical and Physiological Commentaries*, published in 1840, there occur the following remarks:—

"With these habits and means of information, I am now prepared to say, that it is the result of no little investigation that *American Physicians greatly surpass all other nations not only in the decision, but in the success of their practice.* They have followed the Hippocratic rule, that 'severe diseases' require extreme and exquisite remedies,' and so in proportion through all their gradations: nor do they adopt the *expectant* plan, till there is a manifest disposition in nature to take upon herself the completion of the cure. And here I may say her suggestions are equally regarded.

"I have already justified these conclusions as they respect, many of our most prominent writers. Other, and ample records exist to sustain the statement which I make; and placed in a right position, by a charge of imbecility by the morbid anatomists, to illustrate the doctrines and practice of Hippocratists, it has been necessary to contrast their results with those of the school who derive their indications of cure from the *debris* of the body. And, that I am right as to the neglect of all efficient remedies by the school of morbid anatomy, in a general sense, I appeal to what I have already stated in other places, to what will appear in my Essay on the Writings of Louis, and, may I not add to what is proverbially known of the present "expectant" treatment in France, and of the "stimulant" and expectant in Great Britain.

"In America," I also said, that, "practice is not only *heroic*, but remarkably *coincident* in relation to the same diseases; which farther shows that our indications are taken from the proper source."—"There is only a limited school which approximates the Brunonian, in America; and I have adverted to its tenets not only upon principle, but to sustain the comparison I have drawn between practical medicine in Europe and these United States. *I know of no instance, nor have I ever heard of one in* *this country, where a practitioner has been addicted to the 'expectant' plan of treatment.*"

Again, after referring in my Commentaries to the same *kind* of obloquy upon the American medical profession which the Chairman of the Committee, in behalf of the State Medical Society, has heaped upon "ninety-nine out of every hundred" of the American Faculty, and when contrasting the success of their treatment of disease with that of the English and French nations, I said,—"Such is the practice of hundreds in our wilderness. But, they do more; and more wisely. They abjure bark and wine, and carry out a vigorous treatment of inflammations, and of their terribly prostrating fevers, by the lancet. And may I not inquire, also, how all this compares with the dominant treatment, in France, of the same diseases by '*gum potions*,' '*a decoction of triticum*' or of the '*five roots*,' '*bullock's gall*,' '*millipides*,' '*magnetized steel*,' '*a pinch of chervil*,' '*aromatic fumigations under the bed-clothes*,' as set forth in my Essay on the Writings of Louis. Or, is it a less efficient system than the treatment of "the worst forms of fever by *table salt*," as commended and practised by some of the most distinguished of the British Faculty? "*If such patients*," says one of their own commentators, the eminent Beddoes;—"*If such patients had a Rush by the side of their bed, they would not be transferred from it to the grave.*"

'The pleurisy,' says the same English philosopher, 'which I am informed was so destructive among our soldiers and sailors till the *lancet* was used with a boldness almost *totally abandoned in this country*, proves that it is not *American diseases and American constitutions* only that require such treatment.'

'But our own!' exclaims Beddoes, 'The first existing!—Well, then, our own, some of our own, scandalized at the disgrace of the common art, (just as now with our State Medical Society,) did begin to give lessons amain *across the Atlantic.* The lessons, indeed, were received as the *irrelevant effusions* of men who set themselves to *harangue against facts. And American pride was destined ere long to have a fearful revenge. Gibraltar, Gibraltar, is pitted against Philadelphia!*'

"I need not tell you what fearful odds drew from a Briton the honest declaration, that,—'After all British reproaches against the American government and American physicians, for *permitting* the yellow fever to commit such ravages, this yellow fever made incomparably greater proportional ravages in a British garrison, where the authority of medical police was unlimited.' 'And yet again,' he says:—'In the West Indies, on the Continent of Europe, at home, wherever the British Army was posted during the last war, its history of health has offered not only pretexts to a challenged rival, to scorn the arrogated superiority of the British modern medical practice, but also *reasons for an impartial man to pause upon the claim.*"

Such, it appears, was the admitted superiority of American over *British* practitioners of medicine, admitted, too, by the most able and eminent of the British profession,—by Beddoes, Robert Jackson, and others of equal renown,—when there was only one medical school in all America, entitled to any consideration, and but few practitioners who had received the honor of a diploma.

There was, therefore, gentlemen, in those halcyon days, no rankling jealousy, six only of the 180 professorial chairs for the watchful eye of envy, no extensive superiority to be adjusted to the Agrarian system. But, we have now some thirty excellent Medical Colleges scattered over the land, whose chairs are filled, in numerous instances, by some of the ablest men who adorn the medical profession. And what is more, and more to the honor, and welfare of our country, these thirty institutions are annually contributing about one thousand well educated men to the ranks of the profession, whose light sparkles in every hamlet, and exposes the darkness which is now rolling itself into a muttering cloud. Or, shall it be entertained, with the Chairman of the Committee, in behalf of the State Medical Society, that knowledge recedes in proportion to its means of multiplication?

Gentlemen, in justice to myself, and the general cause which I advocate, I must be permitted to make one quotation more from the *Medical and Physiological Commentaries.*

"It cannot be denied," I have there said, "that the Faculty of America, in a general sense, have neglected the literature of the profession, and have been even hurried by the necessities of life into its most responsible occupation without those preliminary qualifications which accomplish the minds of Europeans. This may seem to conflict with what I have said in another place of the advantages of learning. But, it is in no respect an argument against us. Much is owing to the national character of Americans, which is early formed in the most active scenes of life, and renders each in his place a subject of irresistible impulses in all practical pursuits.

"The influence of such habits on the physician I have already shown. American physicians, also, are generally conversant with the writings of those illustrious observers whom I have mentioned as their chief counsellors in the interpretation of nature; and it is only within a brief period that their attention has been at all directed to those physical theories of life which have so extensively supplanted the Hippocratic and Hunterian philosophy in Europe. No obstacle, therefore, having been interposed to divert him from his habits of observing nature, and having no leisure for ambitious views, the American physician has gone on with his former conceptions of the vital nature of disease, and of the operation of remedial agents, and would as soon wait for the plough to move itself in the furrow, as to adopt the *expectant treatment*. I agree, therefore, that a more extended and varied knowledge of true science would have exalted him even higher in the scale of philosophy, and the practical pursuit of his calling.

"In the mean time, let us go on as we have been going,—'*gathering*, *like the bee*, *from abroad*, *but digesting that which is gathered by his own virtues*,' still leaving the *numerical system*, and all others like it, '*to spin out*, *like the spider*, *all its own bowels*,' and thanking '*the empyrical philosophers*, *who*, *like pismires*, *only lay up and use their own store*,' for any proportion of their harvest they may be willing to spare,—being ever willing to receive from the anatomical school their continued castigations for our undaunted energy in the treatment of disease, or their maledictions for waiting upon nature when art has won its triumph. *Esto perpetua!*

"I will not carry out my conclusions beyond the limit of practical habits and results which I had proposed as *a test of medical philosophy*. It is certainly humiliating that I should be so far compelled to depart from any becoming modesty; but, truth must not be suppressed where the most vital interests of the healing art may demand its promulgation. It is right that I should thus inquire into the practical habits of an almost entire profession devoted to the Hippocratic mode of observing nature, and contrast them with the fruits of that philosophy which is founded on the ruins of nature, or seeks an interpretation of her vital phenomena in the crucible of the chemist, or through the glass of the optician."

So far the *Commentaries;* and when, gentlemen, I delivered my Introductory Lecture on *Medical Education in the United States*, in 1843, little did I suppose that I should so soon realize a far more important application of a remark which occurs at the close of that Lecture. The remark is in the following words:—

"Well may we, therefore, spurn the imputations of practical inferiority which have been sometimes heaped upon us even by those who derive their principal title to consideration from being born in the midst of so much enterprise and worth. It is the jealousy of narrow ambition, or the envy of more selfish, or of weaker minds."

And now *again*, gentlemen, for the third time, I stand up *alone*, in the broad expanse of America, in an open defence of the honor and dignity of the profession of which I am a humble member. I stand up to contribute my feeble effort in rescuing my country from the degradation to which its own citizens, they, to whom are committed the honor and respectability of its noblest pride, would consign it in the estimation of the world; and, that I may render this service more effectual, it is my design to incorporate an abstract of this Address in a *Sketch of the History of the Medical Department of the University*, intended for publication when I shall have "gone hence to be here no more."

Nor shall I have discharged the office which I had assigned to myself till I also place on record who they are that malign "the great mass" of American physicians, who are rendering more service to the cause of humanity than any equal proportion of the same profession in the most favored states of Europe.

It is not the man who has officially promulgated the views of the State Medical Society, nor the Journals through which the contumelious representation of the profession is circulated, that should be held responsible any farther than as they, also, hold an influence over the public mind, and according, also, to the *animus*, and the extent in which that influence may be exerted. We must rather go to the fountain from which it emanates, and with acids and caustics try its purity.

We must go to the State Medical Society itself, interrogate the general character of those who annually convene at Albany during the *very opportune* session of the Legislature, inquire *how far*, and *in what ways* they contribute to the dignity of the profession, and advance the interests of medical science.

Nor would I invite an investigation of this nature for the same reasons that I have quoted from Percival's Medical Ethics, were those members of the State Medical Society, who annually convene at Albany, and do the *mining* operations, more than a bare handful of the "*outs*," and were they not so erroneously supposed to represent the voice of the profession. But, that they do not thus convey to the world the sentiments of their "*outside*" brethren, is sufficiently evident from what I have quoted from the organ of the Society. That there are members, however, of this Association who properly regard the transactions of the few, and who leave to those few the *practical existence* of the Society, I am most happy to know; though we must lament the necessity which is thus imposed by self-respect.

But, it may be useful to understand, also, who he is that has thus spoken in behalf of the Society; since we shall the better understand the qualifications of those in whom the Society reposes its greatest trust, and thus, also, become more enlightened as to those members by whom the sacred trust is delegated to individuals of their own body. Such are my motives.

Of the Chairman of the Committee and Projector of the Convention, then, I know nothing but from report, and from what I have seen and quoted from medical periodicals, and from his correspondence with the Medical Faculty of this University. Perhaps my quotations are enough to enlighten you upon the whole merits of the man and his associates at Albany; but since he puts himself forth as the Projector of a National Convention for the *reformation of universal abuses*, and pronounces "*ninety-nine out of every hundred*," of the American medical profession as deficient in the elements of an English education,—so deficient, he says, that when admitted as medical students, it was "*no matter if their primary education should not enable them to write their own names intelligibly*," it becomes worth our while to ascertain how far qualified may be the *Chairman of the Committee of the New York State Medical Society* to be entrusted with the solemn duty of pronouncing judgment even upon the "primary education" of our medical graduates.

This, fortunately, I have it in my power to do by an autograph letter from the Chairman to the Medical Faculty of this University, and which I now hold in my hand. I shall, therefore, read to you a short paragraph from that letter, and then leave the matter to your own conclusions. Thus:—

"The above Circular," says the Chairman, "is an Extract from the proceedings of the New York State Medical Society at its last ANUAL meeting in Feb., 1845. The object of the proposed convention is sufficiently indicated in the preamble and resolution itself. The Committee, in the discharge of their duty, have corresponded with the Medical Societies and Colleges in all the States where such institutions exist, and the replies which have been received are UNANYMOUSLY in favor of the proposed convention.

"Indeed, several Colleges have already appointed delegates; consequently, there is no longer any doubt but that a convention *will be held*. If the subject has not already been before the Faculty of the Medical Department of the University, will you not, without further delay, bring it to their NOTISE?

(Signed,) N. S. DAVIS,

Nov. 25, 1845. *Chairman of Com., &c., Bingampton, N. Y.*"

Now, gentlemen, there certainly appears nothing very remarkable in the extract which I have just read. And yet, beyond doubt, however enigmatical it may seem, there is something *latent* in that extract which shall raise your admiration, and prove to you how well qualified the Chairman may be to sit in judgment upon your "primary education." There are, *O tempora!* in that brief extract not less than three familiar words whose orthography is wrong. "*Unanymously*" is written with two *ys*; "*notise*" with an *s;* and annual is spelt "*a-n-u-a-l*,"—the Chairman having derived the last from the *wrong* Latin word; which shows, also, the extent of his *professional* familiarity with that language of which he accuses "ninety-nine out of every hundred" American physicians with being utterly ignorant.

That this orthography is not accidental appears not only from the frequency with which it occurs, but also from a similar evidence respecting "a primary education" in the Chairman's second letter to the Faculty; where, besides other errors, the word *annual* is again *dishonored* by a single *n*.

Here, gentlemen, I leave this subject; for although I would be just to you, I would be merciful to your accuser. But, there will be no want of commiseration for the champion and favorite of the State Medical Society, if I prove to this audience, and through them to the world, by your own hand writing, that more than *one* in a hundred of you *are able to write your own names*, as denied by the Chairman. But I mean, gentlemen, to prove also, by the same inductive process, far more than that. The hand writing, especially of one's own name, when made under special circumstances, is *an index to the mind itself*, to its acquirements, to its native vigor. You all know how much has been inferred of the talent, and firmness, and conscious solidity of knowledge, from the autographs of the Signers of our Declaration of Independence.

Now, it so happens, whether providentially or not, I am enabled to prove to the world by a single momentary demonstration, that not only *every one of you* can write your own names, but that they are generally *better written* than even the autographs which are appended to the Declaration of Independence. They were written too, under even more embarrassing circumstances; written in a standing posture, and when reputation, and your future calling, and happiness, were at stake;—for they were written at the moment before I began my examination of each one, respectively, upon the *Institutes of Medicine*,—the most difficult and profound of all human inquiries.

It has been my practice, uniformly, throughout the five sessions of this School, to solicit the autographs of the candidates for graduation, at the moment preceding my examination; partly with a view to pleasant reminiscences, and in part, that I might be thus aided in reading the mental condition. Here is the large folio volume in which all are written, and which is open to the inspection of those who are seated around me. They shall bear me testimony, that nearly every autograph proves how little competent is the Chairman of the Committee to appreciate any advances beyond "an elementary education," how undeserved his rebuke of Medical Colleges, how extraordinary his slander of the almost entire profession from which he, and the State Medical Society, derive their *livelihood and their standing in the community*, and, as I doubt not, with the special object of degrading those few who are contributing their time and their best efforts, and sacrificing health, and enjoyments, and professional business, not only for the advancement and diffusion of medical knowledge, but in promoting the general interests of science.

If there be here and there, though rarely, indeed, a name amongst the multitude of autographs, which betrays a faltering hand, time will probably show what their destiny has been, and how true it is that the autograph of a medical student made at the moment which precedes the crisis that is to elevate or degrade his rank, tells us of the state of the inward man.

Gentlemen, the ever sure revolution of time has brought us to the close of another session,—it has brought us near to the last moment when you, and I, and all of us, are about to start upon new scenes, and on other duties; though, God be praised, we have a common avocation, through which mutual recollections, and mutual sympathies, shall daily, hourly, maintain us as a band of brothers, and, in the language of organic science, as one united, harmonious whole.

Aye, my young friends, the pursuit of medicine to cultivated minds is what the *sympathetic nerve* is to the various parts of *organic life.* What is felt by one is thus felt by the rest; and whether healthy or deleterious causes may affect the condition of any part, it is enjoyed, or suffered, by all the rest. Each, in its turn, and all together, conspire in *recuperative effort* for the good of any member that may sustain aught of evil.

Gentlemen, we part; but it is only to cultivate and enjoy the magnificent law of "*remote sympathy*," and to feel that, however distant from each other, we are still but parts of a common whole, and that the most remote member is as near to the whole, and can as speedily reach the whole by a glance of the mind, as the heart can light up a glow of joy through that same mind in its connection with organic sympathy. And should any rude cause offend the "*sensibility*" of your Alma Mater, the great nervous centre,—the *sensorium commune*,—then shall the "*motor power*" as quickly brace every arm to her vigorous defence.

For the present, we part to enter upon the great duties of life, and to carry out the final causes of our being. What an elevated sentiment to know that you are so well prepared to answer the intentions of Providence in his greatest dispensation of benevolence to suffering humanity,—to know that you have entered a field of nature untrodden by other men,—that you are so far in advance of the rest of your race,—which places all others, for weal or for woe, under your magic power,—which assimilates mind and its attainments, more than all things else, to the characteristics of the Great First Cause.

With what cheerfulness should you pursue this noble employment,—with what ardor should you strive to enlarge its boundaries,—with what gratitude should you contemplate the power which you have acquired over the sufferings and wretchedness of your fellow men.

Gentlemen, I part with you with reluctance. Let us part, however, as it becomes men who have pressing duties before them. But let us first interchange pledges of mutual friendship.

Let us pledge ourselves to each other, that we will co-operate in the great work of medical knowledge; nor leave one effort unattempted which may promise a new fact for the great cause of humanity, or which may enlarge still farther our conceptions of Almighty Design in the stupendous system of organic nature. And, finally, gentlemen, when you shall have reached the hall of refreshment, and your weariness, after my long discourse, shall have subsided into the merriment of song, let a glass be devoted to your Alma Mater, and a voice go forth that shall strike dismay into every heart that meditates her wrong.

I could wish, gentlemen, to have seen more of you, to have cultivated more intimately the ties of friendship, and to have been often made happy by more social intercourse. But, gentlemen, the advice which I have given to you, I have ever adopted as rule of action for myself. '*Versate nocturna manu, versate diurna*,' and through sixteen long years of unabating disease, has been my hourly precept to myself. I have toiled both night and day, and more for the public than for myself. We have little time, therefore, for other intercourse than as we meet on the highways of life. Here we greet each other in terms of amity; and our embraces over, we hasten away to do—"whatsoever our hand findeth to do, with all our might; for there is no work, nor device, nor knowledge, nor wisdom in the grave." Happy, however, shall I always be to often renew our pledges of friendship, and to confer with you on the events of life and on the interests of medicine. I hope, too, that we may maintain our social relations, as well by correspondence, as by personal intercourse; and such of you as may be disposed to address me by letter will always find me a prompt and thankful correspondent.

Finally, gentlemen, you know your duties to society, to your profession, to your Alma Mater, to each other.

To YOUR BRETHREN BE JUST, and to mankind at large,
Be ye faithful to all, and all duty discharge.
To each one let 's be kind, to each one yield his right,
On all other topics, let each one try his might.
To superior claims you should gracefully yield,
But only to merit sprung from honor's green field.
To age be respectful, to its faults ever blind,
To your equals be courteous, to inferiors kind.
To woman be gentle,—with a smile greet the child,
And honors shall crown you, and your griefs shall be mild.

THE END.

NEW YORK UNIVERSITY—MEDICAL DEPARTMENT.

A

LECTURE,

INTRODUCTORY TO A COURSE

ON

THE INSTITUTES OF MEDICINE AND MATERIA MEDICA,

FOR THE

SESSION OF MDCCCXLVII—VIII.

BY

MARTYN PAINE, A. M., M. D.

PROFESSOR OF THE INSTITUTES OF MEDICINE AND MATERIA MEDICA IN THE UNIVERSITY OF NEW YORK; MEMBER OF THE ROYAL VEREIN FUR HEILKUNDE IN PREUSSEN; OF THE MEDICAL SOCIETY OF LEIPSIC; OF THE MONTREAL NATURAL HISTORY SOCIETY, AND OTHER LEARNED ASSOCIATIONS.

> "Men are not still the same; our appetites
> Are various, and inconstant as the moon,
> That never shines with the same face again;
> 'Tis Nature's curse never to be resolved,
> Busy to-day in the pursuit of what
> To-morrow's eldest judgment may despise."
> *Southern.*

NEW YORK:
JOSEPH H. JENNINGS, PRINTER, 122 NASSAU STREET.
1847.

New York, November 4th, 1847.

Professor Paine,—

Sir—At a meeting of the Students of the University Medical College, held yesterday, November 3d, H. McNeill of North Carolina being in the Chair, and O. L. Barnes of Georgia acting as Secretary, it was unanimously

Resolved, That a Committee be appointed to request of the several Professors a copy of their Introductory Lectures for publication.

We, who have the honor to constitute the above Committee, would, in behalf of the Class, respectfully request a copy of your able and eloquent address, delivered on the evening of the 28th ult.

With great respect and esteem, we are, yours, &c.

F. A. Stanford, *Georgia.*
G. T. Elliott, Jr. *New York.*
W. A. Hammond, *Pennsylvania.*
J. S. Stiger, *New Jersey.*
J. R. Slaughter, *Alabama.*
M. Whitaker, *North Carolina.*
N. A. Chapin, *New Hampshire.*
H. Burgess, *Connecticut.*
H. B. C. Harris, *Virginia.*
E. W. H. Beck, *Indiana.*
C. McKnight, *Rhode Island.*
W. G. Hatch, *Wisconsin.*
J. G. Buchanan, *Ohio.*
R. L. Crawford, *So. Carolina.*
J. M. Ogden, *East Canada.*
E. Perry, *West Canada.*
Leon D'Alvear, *So. America.*
W. W. Harper, *Louisiana.*
Elihu Edmundson, *Tennessee.*
A. Hallett, *New Brunswick.*
W. J. Rutherford, *Jamaica.*
W. Wallen, *Florida.*
J. T. M'Lean, *Mississippi.*
L. D. Sheets, *Maryland.*
G. W. Peer, *Arkansas.*
W. H. Dial, *Texas.*
J. Poston, *Kentucky.*
A. C. Deane, *Massachusetts.*
G. L. Jones, *Vermont.*
W. K. French, *Maine.*
A. S. Petit, *Illinois.*

By order of the Committee,
O. L. Barnes, *Secretary.*

Messrs. F. A. Stanford, G. T. Elliott, and others,
a Committee of the Medical Class of the University.

Gentlemen,—In answer to your polite note received this day, I shall take much pleasure in supplying you with a copy of my late Introductory Lecture for publication.

Have the goodness to convey to the Class whom you represent my sense of obligation for their courtesy, and accept for them and yourselves my sentiments of respect and esteem.

MARTYN PAINE.

New York, *Nov.* 5, 1847.

INTRODUCTORY LECTURE.

In looking abroad upon organic nature, its most remarkable feature is the variety by which it is distinguished. So great, indeed, is the diversity of form, of organization, and of vital characteristics, the careless observer regards the assemblage as a mass of heterogeneous objects which have few affinities, and often without a remote relationship. He looks upon man as fundamentally distinct from the brute; and wherever specific differences are strongly marked, the same isolation obtains as we descend in the scale of existences. Coming, at last, to the vegetable kingdom, there is so little apparent analogy with the prominent features of the animal race, that none but the physiologist can detect a shadow of resemblance between the two departments of the organic kingdom. How different, however, with him who has explored the whole by the light of science. Whatever the color or the conformation of man, he is always endowed with certain attributes which give to the critical inquirer as perfect an assurance of identity of species as the clearest demonstration enables the ignorant to decide that there is no other difference than color in a brood of chickens. By the same course of observation the philosopher, after descending along the thousands of species which make up the tribes of animals, finds himself wonderfully imitated in form, structure, and functions, by apes and baboons, and taking in his way other species which are as nearly allied to the ape as the ape is to man, he obtains more humiliating resemblances in yet inferior animals; and by thus pursuing the chain of close affinities as one species is only a little removed from the next above, he is ultimately brought to the startling conclusion that he is on common ground with the worm of the dust, as it regards the great plan of organic life. Nay, more, when he penetrates the world of plants, he sees his semblance in every tree, in every herb, and submits to a close alliance with the mushroom and the parasite.

He partakes, in common with the whole, the same elementary composition, the same principle of life, the same functions by which he came into existence, the same by which his growth is carried on, and is finally alike resolved into those elements which he was incapable of uniting into organic compounds, but for the union of which he was dependent upon vegetable structure. He enjoys, however, but only in common with all other animals, a superaddition to his essential or vegetable life in the nervous system and the organs of sense, which are particularly associated with it. In the moral attribute of instinct he offers strong points of resemblance to many of the most humble in the scale of being; but in this respect he is greatly inferior. Man, therefore, finds himself contradistinguished from the vegetable tribes, in respect to the great plan of life, in little else than a more complex structure; and what is superadded to answer the purposes of sensual enjoyment, he holds in common with the kingdom of which he is the head. So far, then, he is only *primus inter pares*,—the first among his equals.

It is true, he walks erect, and has distinguishing powers of articulation, though there are none of us who sing so sweetly as the birds. But who else than man could have traced out this magnificent system of affinities, or have shown his relationship to every moving thing, and to every plant that grows? That is his prerogative alone, and it is that thinking, immortal principle which separates him so widely from all other created existences as to establish a relationship, a companionship with God himself. In this aspect of his nature, he stands alone upon earth, and looks up to heaven for an intimate alliance with that Supreme Intelligence who had laid the plan of his general economy in harmonious relation to those objects which were created for his uses and happiness. In the one, he realizes an infinite wisdom and power in his physical connections which attend his being upon earth; in the other, he equally sees that the end of those connections separates him completely from matter, and leaves him alone related to the spirit in whose image his intellectual part had been ordained and associated with perishable Nature.

It is mine, however, to speak of man as he exists upon earth, to point out his affinities to the objects which surround him, and to indicate the influences and changes to which he is liable. It is, however, only a glance at some small portion of this vast field which can be taken in an hour; and a life-time could not compass what is useful to be known in the walks of physiology, and as they traverse the recesses of pathology and therapeutics. Before we can begin, however, to investigate the functions of living beings in their healthy and morbid states, we must know something of the structure through which they are performed; and this is distinguished by great variety, especially in the animal kingdom. Numerous complex organs are here introduced which have no existence in plants; and where the office of each organ is the same, the structure of each is variable in the different species of animals. It is in plants alone that we meet with little else than what is strictly essential to organic life; and so perfectly co-extensive with animated nature is the fundamental plan of life, that what composes the essential structure of plants is also at the foundation of animal existences. It is in those vessels, or in such as are closely analogous, which carry on the processes of assimilation and secretion in the vegetable world, that we must look for growth, nutrition, secretion, &c. in animals, and what, therefore, is superadded to the organic mechanism of the latter is at most but incidentally subsidiary to the fundamental structure. The same order of simplicity prevails, also, throughout all the mutations to which animals and plants are liable. If disease beset the plant, it is but a modification of the physiological states of the vascular apparatus, and when the latter gives way it is only a restoration of the more natural processes. And just so with man and animals. The diseases of each are only variations of the ordinary condition of the simple instruments of life, and as that condition fluctuates, so will there be health, disease, or convalescence. Whatever may befal a complex organ, it is to its minute apparatus that we must look for the radical evil, and it is there our means of correction exert the salutary changes which they may introduce.

But, the mechanism of which I have been speaking would be as useless as the watch without its spring, were it not moved by a power equally adapted to all the processes of animated nature. The same stillness would prevail in one as in the other. There could be no manifestations of life, without an active principle of life ; and since the results in organic mechanism are every where nearly the same, we unavoidably infer a common moving power throughout,—the same in plants as in the higher kingdom. Thus also we find an universal resemblance in the essential mechanism and in the power by which it is maintained in action.

To this power, for conventional purposes, it is convenient to apply a name. That which moves the mechanism of a watch, we call a spring; and whenever the term is applied in connection with that mechanism, it is suggestive of the office which it performs. I doubt not that all of you regard it as a convenient, and even useful name. The spring of a watch reminds you at once of the mechanism which it controls, and even of what is going forward in relation to time. Coming to the mechanism and phenomena of organic beings, we witness nothing in inorganic nature or among the contrivances of art which bears a resemblance to either of the former. We therefore infer the existence of a power, by which one is moved in giving rise to the other, as unique as organization and its results. This power, for conventional purposes, as in the other case, has been generally called the vital principle, or the principle of life. The term, also, as in the other instance, is suggestive of much of the diversified mechanism to which it appertains, and of very many of those unique results of which it is the efficient cause. It is a term which most men have understood, and have employed as significant of a peculiar power, till quite recently it has been given out that life is a dream of the imagination, and that not a little of all this phantom may be represented by the cunning devices of man. But being determined myself to follow the well beaten path of nature, and to hold on upon Truth, rather than to seek eclat in violations of either, it will become my pleasant duty to place you honestly in pursuit of both, and to show you the snares by which they are surrounded. I speak, however, in a general sense; for there are those who have as honestly imbibed and promulgated the errors which sap the foundation of medicine, and for whom I cherish the deepest respect as philosophers in those departments of science which it is their province to cultivate.

We have now before us, then, a principle of life ;—something, to be sure, not quite so tangible as the spring of a watch, but quite as well understood by the results to which it gives rise; and I suppose in a practical sense, at least, its greatest adversary would make a stout resistance in its defence.

Let us look a little farther at this spring of life. Do you see nothing else than an unvarying movement of wheels; nothing besides some special result as simple as that of the index of a dial? Do you not see the plant uniting the elements of matter into organic compounds, while the animal carries forward those compounds to yet greater perfection? Do you not find a great diversity of secreted products in every plant, and that according to the nature of the species, —according even as every part differs from another part,—and that, too, forever the same in the natural state of the being? What variety of odors, what an exact but endless variety is offered to the taste and to vision! What strange diversity, yet always precise; while the action of this almost endless series of organic products is always the same upon every part to which it belongs; and yet that which is the product of one part is often destructive to all other parts of the same being by which it is generated. But we have no time to pursue what is apparently without end. The brief suggestions which I have made will remind you of a thousand others of a similar nature, and satisfy you at your very first step in the vegetable kingdom, at the very threshold of life, that you have entered a world which has no analogies in composition, in structure, in powers, in functions, in tangible and in intangible results, with any of the conditions or phenomena of inorganic nature. Glancing again, at the animal kingdom, it is all the same as respects the essential conditions of life, the same exact variety, the same functions, products, and other endless phenomena, with smaller differences in respect to each than such as distinguish that elaborate structure where all the important processes are carried on.

You begin, therefore, to realize the necessity of a peculiar power for all this unique variety, and the convenience of some term which shall distinguish it from every other power, and which, as far as possible, shall be expressive of its important offices. You are already prepared by this superficial view of the beginning of our demonstrations to take up your steady march along this fascinating path of nature, to be conducted along rather by the steady light which she may afford, than by those gleams or flashes with which art may attempt to beguile your imagination. I freely concede that one is a task, while the other is a pastime; that one involves the widest and a toilsome research, while the other is so purely a matter of sense as to admit of nothing else than an agreeable exercise of vision. Such of you as may prefer the former, will become enlightened physicians, will find in organic inquiries the best of your enjoyments, and will realize in yourselves what gives the highest value to man; but such as will be satisfied with the illusions of sense, as they abound in the outskirts of medicine, will become the victims of sense, and your patients the victims of error.

As you advance in the knowledge of Physiology, you will see that the effects of life are so various, and so obviously influenced by natural agents, and even by what is within—by the mind itself—you will necessarily conclude that the principle of life is also unlike all the other powers in nature in being endowed with certain properties, and liable to certain changes, which are totally unknown to the inorganic world. You will see, for example, that this principle is variously acted upon—and according to the nature of the agents, and that motions and other effects ensue more or less in conformity with the influences which are exerted. These phenomena have given rise to an analysis of the principle of life; and practical uses as well as philosophy have ascribed to it a property of irritability, as well as of mobility;—just as they have to the soul the properties of judgment, reflection, &c. Mobility implies the power of acting, and is a very convenient name among those who are inclined to understand each other. Irritability has been long in use to denote a peevish mind, and by a little modification of its import in that relation, we shall find it a very convenient and useful term to denote the property in organic life upon which all things make their impression, and through which the moving power brings the mechanism into action.

So far, then, and much farther, all things are common to plants and animals; the whole assemblage of which constitutes their essential or organic life. But there are certain things peculiar to animals, and, therefore, as there is reason to believe, are totally wanting in plants. The latter, for example, neither see, nor hear, nor smell, and these are functions which many are apt to suppose are the very essence of life. But this is a very false conclusion, for the animal would live just as well without eyes, nose, or ears. This is distinctly seen in the condition of the fœtus, and even during sleep; for then organic life is alone in operation. Looking, also, at the internal structure, we find a remarkable system of organs in animals, of which no trace can be detected in plants. That system, according to its different parts, goes under the names of brain, ganglia, and nerves, and it is found to be especially subservient to the senses, and to the soul and instinct. It also establishes harmonious relations among the more complicated organic structures of animals; and it is a medium through which that harmony may be disturbed. There is nothing like this in plants; and yet they have as much of what is truly essential to life as the most perfect animal. What is superadded to the latter is for his convenience, his enjoyments, and to balance nicely his more compounded structures.

The peculiar functions of which I have now spoken are assembled into two genera, one of which is called sensation, and the other sympathy. Sensation comprehends seeing, smelling, hearing, tasting and feeling; while sympathy is the office by which harmonious relations are established among the complex structures. You perceive that they are very good names, and are quite expressive of what they are intended to mean; though here, as with every thing else which implies the existence of life, many have been disposed to quarrel them out of the language of science.

Thus we have got two comprehensive functions which are peculiar to animals,—sensation and sympathy.

These functions imply the existence of two properties of the principle of life, which are as peculiar to animals as are the functions which originate in them. One of the properties is called sensibility, the other the nervous power. Sensibility, therefore, is the property upon which sensation depends, and the nervous power is the agent of sympathy; while the nervous system is the part or organ in which they reside. You perceive, also, that these are very good names, and are very significant of what they stand for. It is true, that many have made the great mistake of supposing that the nervous system is intrinsically concerned in the organic processes of animals; but since those processes and their results, such as growth, secretion, &c., are essentially the same in plants as in animals, and as plants are destitute of nerves, and possess no nervous power, you readily see the nature of the blunder. Finally, sensibility is a good deal allied to irritability; since, while all things make their impressions upon irritability in absolute life, it is upon sensibility that agents operate in giving rise to sensation. Thus, for example, it is upon the irritability of the heart and blood-vessels, and upon the sap-vessels, that the blood operates in one case, and the sap in the other, and thus maintain the several parts in action. Cathartics do the same in respect to other organs, and heat acts upon the same property throughout the universal body both of animals and plants. And so of all things else in the main department of life. Coming to sensibility, this is acted upon in the retina by light, in the acoustic nerve by the vibrations of the tympanum, &c., in the Schneiderian membrane by odors, and so on. You readily see, therefore, the distinction between irritability, which is common to plants and animals, and sensibility, which is peculiar to animals.

What I have now said of the superaddition of certain organs, properties, and functions to animals, has given rise to a division of their life into two parts; one of which embraces what is truly essential to life, and belongs equally to plants, and is called organic life; while the other, or non-essential, and which is peculiar to animals, is called animal life. Nevertheless, it should be understood that such parts as are most essential to organic life pervade all the organs which compose the division of animal life, since growth, nutrition, &c. are as perfect there as in plants themselves. The nervous system, also, being rendered subservient to the organic life of animals, is carried into all parts of their organization; although the cerebro-spinal system belongs, intrinsically, to the division of animal life. The nervous power is thus rendered an agent by which all parts are balanced in their healthy functions, by which all parts are rendered sensitive to the condition of each other, and by which, when one becomes disturbed, another, or all other parts, may be thrown into disordered action. In the natural state, for example, if the skin be chilled and perspiration checked, the nervous power immediately excites the kidneys to an increased secretion of urine. Another plain example of an analogous process occurs in every act of respiration, and the process of respiration exemplifies exactly, what is, or should be meant by sympathy. The mind and its passions are also constantly bringing the nervous power into an endless variety of influences, both in organic and animal life. In the former case, we see its operation directed upon the stomach when vomiting is brought on by the imagination, and upon the capillary blood-vessels of the face, when shame or anger suffuses the countenance. There is nothing like all this in plants.

We have thus a great symmetrical system, in structure, properties, functions and organic results, which is alike common to all animated nature, while certain additions are provided in animals to answer some special ends of their being. You all see that it is a consistent, a harmonious plan, and that it is only when we depart from the obvious path of nature, that incongruities begin to appear. I say you are already convinced that truth in physiology is just as simple and as easily comprehended as it is every where else. To beget conviction, it is only necessary to present it in its naked simplicity, and it will then be self-evident to any mind that has not entangled itself in the prejudices of error. Hence, too, you will readily appreciate the importance of beginning right, and with a determination to reject whatever conflicts with the self-evident propositions of truth. Whatever infringes upon the consistency and the unity of the great plan of organic nature, you may depend upon it, is the spurious work of man. I do not mean, however, that you should turn your sight from error as it dances before you; for whatever is dignified in truth will always gain by any just comparison; and you should know the false that you may assist in restraining its progress.

It is true, the school of eclectics, the *In-Medio-tutissimus-ibis* school, will tell you that it is their province to sift the wheat from the chaff, and that you have only to hold your hats as the grain runs out; that theirs is the school of order, of harmony, of modest pretensions; that Truth is in their disinterested and holy keeping; that they alone can be trusted.

> " They hang between, in doubt to act or rest."

Such, you see, gentlemen, is my solicitude for your safety, that I have again wandered from my subject to show you the importance of not departing from it yourselves. But I can only now present you the great landmarks which should guide your steps throughout all that domain of nature which it is our province to cultivate. Nor have I done more than make a general survey of animated existence as presented in its most natural aspects. Looking at this alone we should imagine that it is all without change, and that every living being is destined to live on forever. There is nothing in the perfect state of animals or plants which denotes their mutability beyond what is incident to growth and nutrition,—nothing of the liability of all to disease, or to death. All this is inferred from another series of observations;—and here we pass into the vast fields of pathology and therapeutics. But there is no possibility of entering those regions but by the great domain of physiology. It is true, shorter cuts have been often attempted, and in recent times it would seem almost as if they had been overrun by foes and plundered of all that is valuable, or which entitles them to our respect or attention. Your help is wanted in maintaining the integrity of nature; in repairing the breaches that have been made in the bulwarks which she has erected. You will find much in the artificial systems of physiology that is so estranged from nature that you will have no chance of smiling even at a clumsy caricature; and when you turn to pathology and therapeutics, as managed by the same philosophers, you will be amazed to see how these three branches of science have been stripped of their relations to nature. Should you, however, be inclined to follow those inquirers who have been guided by the light of truth, you will find all my assurances sustained by your own observations. You will find nothing inconsistent in any branch of your pursuits, and that the whole is bound together by the closest affinities. You will find that physiology, in its connection with organization, lies at the foundation of pathology and therapeutics, and of all those intermediate changes which make up the transient or permanent differences among individuals of the same species. All the changes that may befal the most natural state of the being, from the most aggravated forms of disease to temperament itself, are intrinsically nothing more than the physiological states more or less turned from their natural standard; while therapeutics is only the method of turning them back again. For great and wise purposes, the properties of life are rendered mutable, and as one cause or another, and according to its virtues, may make its impressions upon irritability or sensibility, so will it be felt, and corresponding effects will follow. If it be miasma, fever may ensue, and then, perhaps, a cathartic, or an emetic, by a different impression, will place nature in the way of passing again to her ordinary state. All the changes, too, which constitute the different forms of disease are attended by such modifications of irritability and sensibility, that the subjects of such changes are very differently affected by physical agents than in the condition of health. All this, too, will be according to the combined circumstances which make up the nature of the change; and it is the finding out of these circumstances in every case of disease, and at all stages of its progress, and adapting our means of cure in conformity with them, which form the greatest difficulties in practical medicine.

Now, gentlemen, this mutability of the properties of life is at the very foundation of the healing art. When they are driven from their natural standard to a morbid state, it is more or less their tendency to return to their healthy condition. This tendency may be often greatly promoted by art; but in many instances, as in the self-limited diseases, it so far transcends all artificial impressions, that, in a general sense, it will admit of little or no interference. This great law, therefore, is the very basis of medicine. Without it, remedial agents would be powerless; the knife of the surgeon, and his caustic and poultices, would have had no existence. It is the sole dependence of plants and of the brute creation. All animated nature, indeed, would utterly perish without it. Galen in one line expresses beautifully the whole extent of the doctrine. "*Natura malum sentiens gestitat magnopere mederi.*" *Nature cast down desires greatly to be assisted.*

The general tendency, of which I have now spoken, in the properties of life to return from their morbid to their natural state, whether spontaneously or brought about by art, has been long known as the *Vis Medicatrix Naturæ—the recuperative power of Nature.* You perceive, gentlemen, that this also is a very good name, so only you agree to understand alike its proper import. None understood it better, or has expressed it better, than the father of Medicine. "*Natura deficiente,*" he says, "*quicquam obtinet media ars, perit æger.*" *If nature come not to the aid of the medical art, the sick man dies.* And Celsus to the same effect. "*Natura repugnante, nihil proficit medicina.*" *If Nature do not co-operate, medicine is useless.* Or, as the poet has it,

> "When Nature cannot work, th' effect of art is void,
> For physic can but mend our crazy state,
> Patch an old building, not a new create."

I have said that physiology is so completely at the foundation of all the changes which befal the living being, that the same great principle stretches from disease to those differences among mankind which are known as the temperaments, and even to the changes which are effected in plants by cultivation, by changes of climate, &c., and in animals by analogous influences. And here I have thought that I cannot do better than to illustrate this subject by a brief analysis of the temperaments of mankind; and for this purpose I will be indebted to a page which I have already placed before the world, but too recently, however, to have engaged your attention.

The temperaments may be regarded as embracing the innate as well as acquired peculiarities of constitution; for although the latter depend upon causes that are relative alone to the individual, the former or innate constitution has been brought about, at some anterior generation, by the physical agencies of life. This is the true temperament, and belongs to masses of mankind.

Idiosyncrasy is only a variety of temperament and constitution, and like those, therefore, depends upon some peculiar modification of the properties of life, especially irritability; but only so in relation to a very few particular agents. It is peculiar to individuals, rather rare, and may be hereditary or acquired. This peculiarity is not unfrequently the cause of the favorable or deleterious effects of certain remedial agents, of certain kinds of food, &c. We see the important principle illustrated every day, every hour. Here is a subject who is salivated by the external application of a few grains of mercurial ointment, and in whom various diseases may be speedily extinguished by this simple use of the remedy. But here is another, in whom the internal administration of an ounce of calomel may produce no constitutional result, and make no impression upon disease. Or, it may be in another case of extreme susceptibility to the action of mercury, that the agent always displays the effects of a profound poison, aggravating fever and other affections, or, in the absence of disease, greatly deranging all the functions of life. Most men are poisoned by the slightest contact with the Rhus vernix; but now and then an individual handles it with impunity. Muscles and some other animals are always poisonous when eaten by some people, though generally good articles of food.

Constitution comprehends all the peculiarities of the individual; the temperament, idiosyncrasy, conditions relative to age, sex, habits, &c. It is, therefore, liable to many variations at all periods of life. The prevailing characteristics of each of the elements may remain, but yet so modified, that what is known as constitution may be "broken down."

The same principle is concerned throughout, whether in respect to constitution, temperament, or idiosyncrasy. It is the same as prevails habitually in respect to the naturally modified irritability of different organs in man, and in all animals, and in plants; that which renders the product of one organ innoxious to some parts, but morbific to all other parts,—that which renders the eye susceptible to the undulations of light, the ear to the undulations of air; and so on. The principle, and its everlasting, unchanging laws, are every where, in all that relates to organic beings, whether in respect to the system in its abstract condition, or as relative to external agencies. It is a great and wonderful principle, a perpetual study for the philosopher, ever pregnant of variety, ever illustrative of the peculiar character of the properties of life, of their natural modifications, of their instability, and forever supplying fresh sources of interpretation of the laws which the properties and actions of life obey.

It is evident, therefore, that temperament, constitution and idiosyncrasy are constituted by certain acquired or transmitted conditions of the vital properties, which form a part of the natural or habitual state of each individual, and from which arise various degrees and kinds in the susceptibilities to the action of physical agents, and certain peculiarities, also, in the material condition and conformation of parts, especially the external. By studying these sensible peculiarities, as well as the phenomena of life in their natural and morbid conditions, we infer the peculiarities of the natural vital conditions in different individuals, or their natural constitution and temperament, or any more remarkable idiosyncrasy. They reach, also, to the mind, which is apt to bear certain relative peculiarities to those of the organic states.

In the farther consideration of this subject, I shall regard those peculiarities of constitution which are mostly of a determinate character, and include them under the general denomination of temperament.

The physiological differences between temperament, idiosyncrasy, and constitution, are neither great, nor of much practical importance. Indeed, so allied are they in principle, that a common philosophy determines the remedial treatment, which is always more or less modified by temperament. Each should be considered along with the modifying influences of habits, climate, &c.

Temperament and constitution do not depend, as supposed by some writers, upon the special development of particular organs; though this is true of some of the vicissitudes of age. The former have their foundation in the system at large, and are apt to be transmitted by one or by both parents; or, the transmitted peculiarities may come from a remote ancestor, and not from the immediate progenitor. This last peculiarity is analogous to one of the characteristics of the scrofulous diathesis, where it passes over one generation and reappears in the third.

It appears, therefore, that temperament, whether innate or acquired, is due to the slow operation of causes upon the vital constitution, just as it is in respect to the habitual use of tobacco, of opium, &c., or as it respects certain morbific causes.

In the latter case, the modifications are more or less transitory; but may be so ingrafted as to be transmitted, for a time, like the permanent temperaments, from parent to child, as seen of some diseases, such as rheumatism and gout, or of predispositions to diseases of a transient nature, as in small-pox, or even ordinary fever. Coming to hereditary diseases of a permanent nature, as scrofula, we run from the transitory phenomena of vital habit, which respects the use of tobacco, opium, &c., by an intimate analogy, into the permanent temperaments; and from these we are conducted by the same philosophy, which respects the operation of physical agents in modifying the properties of life, to those more remarkable peculiarities which spring up in animals from domestication, and in plants from changes of climate and soil.

It is scarcely probable that differences in temperament have, often, any appreciable effect on the elementary composition. Differences, however, obtain in respect to structure, as seen in the general form, the proportions of the limbs, the features, &c., while more remarkable corresponding analogies are witnessed in the herbaceous and arborescent habits of the same plant, as it may be subject to the influences of a tropical or cold climate, as the Ricinus communis.

Great differences arise not only in respect to the influences of the same remedial agents, from the mere circumstances of temperament, but morbific causes may be equally various in their operation. The same causes may be very apt to affect one temperament, while they will rarely have an effect on another temperament.

The temperaments as designated by the ancients, and retained by the moderns, are divided into the Sanguine, the Melancholic, the Choleric, and the Phlegmatic. The artificial habits of the moderns have added a fifth, or the Nervous.

It is not usual to find all the attributes of each temperament united, while some of the whole may be blended in the same individual. Nevertheless, the characteristics of one or the other generally predominate.

Temperament is most distinctly pronounced at adult age.

1. *The Sanguine Temperament.*—Unlike the other temperaments, the characteristics of the sanguine are perpetuated from infancy, and perhaps, therefore, may be considered the most natural. The skin remains soft and delicate; the limbs rounded, and full; the superficial veins, unlike those of infancy, large, conspicuous, and blue, especially about the head and temple; the complexion fair, florid and animated; the eyes large and blue; the hair light, or red, or of intermediate hues.

Sensibility and irritability are strongly pronounced; the great development of the latter giving the principal determination to the sanguine temperament. The blood, in consequence, stimulates the heart to more frequent, high, and regular action, maintains the capillaries in a lively and plethoric state, and thus determines the redness and softness of the skin. Other vital stimuli, also, operate with greater intensity than in other temperaments. For the same reason, the secretions and excretions are rapid and copious, and are little liable to vacillation in the ordinary conditions of health. All things else move on in a corresponding manner; the whole assemblage of which beautifully illustrates the true philosophy of life.

The great development of sensibility contributes, also, its considerable part to this temperament. The senses are ever on the alert; and here, as with irritability, external objects make their impressions with great effect and rapidity. Perception is rapid, reflection quick, imagination lively, memory prompt. The succession of ideas is too rapid for comparison, and hence the judgment is infirm, unless associated with genius; when it is distinguished for eccentricities. This is exemplified in the poet, Byron, and in the warrior, the Marshal, Duke of Richelieu,—"that man so fortunate and brave in arms, light and inconstant, to the end of his long and brilliant career."

Inconstancy and levity are the great moral attributes of the sanguine. Variety and enjoyment never satiate. Devoted to sensual gratifications, they are in love with all female beauty, and are inconstant to a mistress, if not to a wife; yet are they honorable in all things else.

The sanguine is eminently generous or prodigal, and the end of gain is the purchase of pleasure. Quick in anger, he is soon cool, or he is impelled to hasty decisions that are soon regretted. A challenge to a duel would be gladly abandoned, did not a sense of pride urge him on to the combat. Revenge and envy have no hold upon this constitution.

It is evident, therefore, that the prevailing diseases of the sanguine temperament are inflammatory; that the organs sympathize readily and greatly with each other, and that the sympathetic affections are disproportionately greater than the primary affections. Infancy always partakes of this temperament; but if it be truly constitutional, the infant is liable to extraordinary demonstrations of its fundamental nature. The irritation of a tooth, for example, is more apt to produce convulsions, and intestinal derangements still more so, or to lay the foundation of cerebral diseases, &c. Anger being quick and vehement, here displays its instant effect in developing inflammations and hemorrhages. But love is instable, and as envy, grief, and jealousy torture not the mind, so do they not the body.

As external causes, whether natural or morbific, make their impressions rapidly and profoundly upon the sanguine temperament, and its diseases being active and violent, remedial agents should be prompt and decisive, as in infancy; but here, also, for the reasons which are relative to the first period of life, remedies are also profound and speedy in their operation. And since the prevailing disease of this temperament is inflammation, bloodletting is the principal means of cure, and will require but little co-operation from other agents. If early applied, and carried to its proper extent, it will nearly extinguish the most violent inflammations during its first application. The test of this extent will be also more exactly determined in this, than in other temperaments, by the subsidence of symptoms during the progress of the operation. It is in this temperament, also, that the philosophy of the vital influences of loss of blood is most evidently shown.

2. *The Melancholic Temperament.*—The melancholic temperament has certain points of resemblance to the sanguine, though they are strongly contradistinguished. The general external aspect of the sanguine is cheerful; that of the melancholic, dry, stern or gloomy, and excites no liveliness in others, though it command respect, and

even admiration. The solids predominate in the melancholic; the capillaries show less blood, though the veins are large and more prominent, but less transparent than in the sanguine; and unlike the latter, the skin is darkish, or inclining to yellow, thick, coarse, and hard to the lancet. The blood flows more freely from the sanguine when the skin in pricked; and this exemplifies the state of the capillary circulation at large. The same principle obtains, therefore, in the pulmonary circulation, and hence in part, the blood is darker in the melancholic than in the sanguine. The eyes of the former are darker and less prominent than in the latter; and the hair is dark, coarse, or stiff, eye-brows large, black, and often projecting; the muscles and tendons, like the superficial veins, stand out, from the absence of that cutaneous fat which gives rotundity to the body of the sanguine.

It is easily seen, therefore, that irritability and sensibility are comparatively dull in the melancholic. External objects do not make the strong and rapid impression upon the senses as in the sanguine; and from the obtuseness of irritability, the action of the heart is slower, the capillary blood-vessels are less charged with the vital fluid, the secretions and excretions less, and more slowly performed.

The melancholic temperament is the principal abode of genius; embracing a large proportion of those great men who have unfolded the laws of nature, or have made the highest advances in the arts, or have astonished the world with deeds in arms, or with the achievements of the statesman, or the orator, or the painter, or the poet. Here is witnessed the highest intellectual renown at the very dawn of manhood; and here it is that we often meet with genius struggling with those adversities which arrest the ambition of other temperaments. The melancholic is forever indomitable; rising in determination as obstacles rise before him. Inflexible in purpose, the passions are disciplined to urge on an arduous enterprise, or, if allowed to become impetuous, it is to accomplish the decisions of the understanding. With equal facility he concentrates his mind upon abstract inquiries, or at the next moment, sends it abroad over the widest theatre of its operations. He is bold and brave, never fearing death, nor wantonly incurring danger. He moves steadily forward, though he does not move, until he has explored the path before him. His imagination, therefore, is of the highest order, being disciplined by the sterner faculties. It is such an imagination as is always an element of genius; such as contemplates the realities of life and the truths of revelation. He is thoughtful, grave, or sad, but may turn his mind to great elevation, and great sublimity and enthusiasm, and often soars on poetic wings through the regions of heaven. The sanguine on the contrary delights in the *romance* of fiction.

Honor holds its empire in this temperament, however it may be wanting in human sympathies. If pledged to a good or bad action, it is fulfilled. The melancholic is generally fervent but dignified in his attachments, or looks with indifference or with scorn upon humanity. A few, like Tiberius, are fearful, perfidious, suspicious, and cruel; and others, like Nero or Richard, insensible to danger, and ever ready for the work of death.

As with sensibility and irritability in their natural aspects, so it is in their relation to morbific and remedial agents. The coincidence is universal. The former are slow in establishing morbid changes, many are inoperative which readily light up the flame of disease in other temperaments; and the passions are subdued by the melancholic into mere agents of the understanding. But when morbific causes have made their impression, the dullness of irritability and mobility explains why disease is apt to be obstinate, and why remedial agents operate with less rapidity than in the sanguine. The vital properties and functions being slowly susceptible of morbid changes, they are slowly altered from their morbid states.

It is easily inferred that the diseases of the melancholic are mostly of the digestive organs, and that their removal is tedious. It is also manifest that these, and other affections, are slow in developing diseases of other parts, and that the brain and the mind must be most likely to sympathetic disturbances. Hence it is that hypochondriaism and insanity are apt to supervene in the melancholic temperament.

Cathartics are demanded more by the melancholic than by any other temperament; though their exigencies have a special relation to the disorders of the digestive functions. Bloodletting, also, is often necessary to reach these chronic maladies; and although its delay in the grave forms of inflammation be less hazardous than with the sanguine, its necessity is as great, and its extent and frequency of repetition are greater. It is here, too, that the greatest demand is made upon the materia medica for auxiliary means.

3. *The Choleric Temperament.*—The choleric is intermediate between the sanguine and melancholic temperaments; and although it form the sanguineo-melancholic, it possesses characteristics which give it an individuality. The skin has greater fullness of the capillaries than in the melancholic, and therefore greater softness and warmth, but less than in the sanguine. The pulse is intermediate in fullness and frequency. The secretions and excretions moderate and uniform. The healthy functions performed with regularity and ease. The choleric is tenacious of his own rights, but less disposed to infringe upon the rights of others than the melancholic, while he has less generosity than the sanguine. The higher faculties of the mind correspond with the other characteristics of this temperament, being generally distinguished for their moderation.

Irritability and sensibility holding an intermediate degree between those of the sanguine and melancholic, external agents operate with a relative effect and rapidity; so that the organic functions move on without frequent or profound interruptions, and diseases yield to a more compound treatment, though less readily than to the simpler means required by the sanguine, but more speedily than in the melancholic.

4. *The Phlegmatic, or Lymphatic Temperament.*—The phlegmatic is characterized by slothfulness of mind, and by a simpler display of vegetative life than any other temperament. The flesh is soft, the countenance pale, the hair delicate, and the fat amounts to an incumbrance. The limbs are rounded, feeble, and without expression.

The veins are small, and lie deep. The pulse is small, feeble, and soft; arteries small, and the capillaries deficient in blood. Irritability is dull. Sensibility is obtuse, and perception weak, which greatly circumscribes the senses as an avenue to the mind; while

> "Fat holds ideas by the legs and wings."

But, with all the intellectual dullness and bodily indolence, which distinguish this temperament, it is obstinate, fearful, suspicious, and avaricious.

The organic functions of the phlegmatic are easily liable to interruption, though morbific causes, unless intense in their nature, make their impressions feebly. The mind, and its predominant passions, have, of course, but little agency in the production of its diseases. Disturbances, however, seem to arise from the mere inertia of the vital powers; and when morbific causes make strong impressions, the properties of life often go down, at once, to near the verge of extinction. So, also, do active remedial agents operate with a relative effect. Emetics are scarcely admissible; violent cathartics prostrate excessively; and any unnecessary extent of bloodletting breaks down the whole energies of the body. This temperament therefore requires great moderation of treatment.

5. *The Nervous Temperament.*—The nervous temperament displayed itself feebly among the ancients, but has been brought to a high maturity by the progress of civilization. It is the only temperament where the primary causes may be traced, which consist mainly of such as are attendant on indolence and sedentary pursuits. It involves alike therefore, the rich and the poor, the sensual devotees of fashion and the plodding shoemaker, the laborious student and the readers of romance.

The nervous temperament is founded upon the sanguine, or the sanguineo-melancholic, and is either transmitted, or springs up originally in the individual. It is therefore the most artificial of all the temperaments, and is susceptible, individually, of great improvement. It is shown externally by a general aspect of feebleness, a spare body, and small, soft muscles, which are incapable of much exertion.

An unusual predominance of sympathy is the leading characteristic. Irritability is also strongly pronounced. Hence, slight disturbances, even of unimportant parts, give rise to greatly disproportionable sympathies in the more important organs; and these secondary results will be still more intense if the primary disease be seated in any important organ. The functions are constantly subject to irregularities, especially those of the abdominal viscera. If the subject be addicted to the causes of this temperament, he is rarely free from indigestion, and an attendant train of other evils, according to the nature of his indulgences or pursuits.

Diseases, however, are not as violent as with the sanguine, nor as profound as with the melancholic. The mind is irritable, but the passions not violent, though they readily disturb the organic functions. Such as display themselves depend much upon the habits and occupation of the individual.

Remedial agents operate with power, the same coincidences existing between their effects and those of a morbific nature, as in other temperaments. Moderate impressions, therefore, made upon the alimentary canal are sensibly felt by remote parts; and in this temperament, particularly, the peculiar principle upon which leeching operates is well illustrated.

What I have now said, specifically, of the temperaments is liable to certain qualifications.

Different epochs of life appear often to partake of a particular temperament; one subsiding into another. The sanguine is most characteristic of infancy and childhood, the melancholic and choleric of middle age; and the phlegmatic of old age.

The several temperaments are also often blended, more or less with each other, in the same individual. When thus combined, they are called the sanguineo-melancholic, the sanguineo-phlegmatic, &c.

They are also liable not only to the foregoing modifications from age, but from sex, climate, habits, education, &c. So great indeed is the influence of climate, that a change of residence (as from a northern to a tropical country) will sometimes gradually transmute one temperament into another; and this is particularly true of the sanguine, the melancholic and the choleric.

Accidental influences are sometimes such as to generate anomalies, in which it is difficult to recognize any distinct features of the prevailing modifications of temperament, and which may disappear with the individual, or be transmitted to his descendants.

All the varieties which I have now stated, are more or less liable to modifications of a common form of disease, and require corresponding variations in the details of treatment. They concur together, therefore, in forming a part of the difficulties of medicine, and in demonstrating the complete abstraction of organic beings from the forces and laws of the inorganic.

I say, organic beings in their most comprehensive sense. For are not the varieties which have sprung from domestication and cultivation, among animals and plants, and which are equally, and more perfectly transmitted than temperament, constitution, &c. in relation to man, integral parts of a common principle? Exactly the same philosophy lies at the foundation of the whole, and is another broad field of evidence to substantiate the unity of the vital principle, of its common laws and functions throughout animated nature, and presents the whole in a magnificence of grandeur, a harmony and unity of unfathomable designs, which forms an unutterable contrast with the physical hypotheses of life.

> "Thus, then, to man the voice of Nature spake—
> Go, from the creatures thy instructions take;
> Learn from the birds what food the thickets yield;
> Learn from the beasts the physics of the field;
> The arts of building from the bee receive;
> Learn of the mole to plough, the worm to weave;
> Learn of the little Nautilus to sail,
> Spread the thin oar, and catch the rising gale."—*Pope.*

CONTRIBUTIONS IN PHYSIOLOGY BY DR. PAINE.

THE CIRCULATION.

[Continued from vol. 38, page 527.]

To the Editor of the Boston Medical and Surgical Journal.

SIR,—At the close of my last communication, I remarked that, "at another time I will briefly continue this subject, for the purpose of showing that mind is a perfectly distinct essence." Since then, I have concluded to examine the question in the form of an essay, and will now, with your permission, extend my remarks, as embraced in the foregoing article, upon the subject of the circulation in plants.

Absorption by the roots is considered an inadequate explanation of the circulation of sap among those who advocate the doctrine of capillary attraction; and, if numerical strength were to decide the question, what I am about to say would be a waste of your valuable Journal. To interpret the process in plants, the leaf, or its equivalent, has been taken as especially instrumental; serving either as an exhausting apparatus by evaporation, or under the designation of endosmosis, or contributing its aid by supposed chemical influences, through the operation of light, upon the ascending sap. Some one of these hypotheses is considered an indispensable auxiliary to the doctrine of capillary attraction as applied to the circulation of sap.

An ingenious application of chemical principles has been propounded to satisfy the supposed exigencies of capillary attraction as concerned in the absorption and ascent of sap, and professes to afford the true solution of the downward motion. This hypothesis is also thought to be a new obstacle to the doctrine which ascribes life to a plant, and the dependence of its circulation and unique products upon vital actions. The hypothesis derives, also, no little importance from its application to the circulation of the blood, and the admission that, if it cannot be sustained in respect to plants, it must be equally groundless in regard to animals. The doctrine comes recommended to our attention by its distinguished advocates.

I have already endeavored to show, extensively, the want of all foundation for the more comprehensive principle set forth by Liebig, that—

"*The* CAUSE *of the state of* MOTION *is to be found in a series of changes which the food undergoes in the organism, and these are the results of processes of decomposition, to which either the food itself, or the structures formed from it, or parts of organs, are subjected.*"

This summary principle, in which oxygen gas figures conspicuously, is the combustive doctrine of life. The projector held it to be applicable to every motion and to all the phenomena of living beings, in health and disease, and even in death. It was also made to explain our very thoughts and passions; these being imputed to the union of oxygen with the combustible elements of the brain.

I cheerfully conceded that—"this summary principle, were it true, would be truly beautiful." I therefore felt the importance of showing that "it was not only deficient in every necessary element, but was contradicted by all the phenomena of sympathy and by all that is known of pathology and therapeutics; and this, too, according to the author's own repeated and fullest affirmations and doctrines to the contrary." This I endeavored to execute particularly in my articles upon Digestion and Animal Heat; and I am thus provided with a vast series of facts in advance, which must be taken in connection with what I may now say of the corollary from the fundamental doctrine. This corollary consists in the application of the general doctrine to the circulation of the sap and the blood. It supposes that the movement of the sap, upward and downward, is generated in the leaf by the action of light in promoting the decomposition of carbonic acid gas. The imperfect ascending fluid is thus converted in the leaf into perfect sap, and the change is supposed to institute a propelling force in the imperfect juice, by which the elaborated sap is driven out of the leaf and through its downward course. The force, generated in the leaf, is also considered, from the motion which ensues in that part, as the most essential cause of the ascent of the sap, or that the fluid is thus *lifted* from the roots to the summit of the most lofty trees.

The same principle is applied to animals. The pulmonary circulation is said to depend upon the union of the oxygen of the air with the carbon of the venous blood, in consequence of which this blood drives the decarbonized into the left auricle. But in the case of the systemic or greater circulation, the order of things is reversed; for here the motion is supposed to be generated by the union of oxygen with the "structures formed out of the food." The same order of events obtains in the liver—all referable to "a series of changes which the food undergoes in the organism," &c.

I shall now limit my remarks to the supposed condition of the circulation in plants; since, if the hypothesis can be contradicted here, it must equally fail, as admitted, in respect to animals; and I shall endeavor to avoid whatever I may have hitherto said. Perhaps, however, it may be well to state the confidence which is entertained in the equal applicability of this theory to plants and animals, and the admission of its necessary failure throughout should it appear defective in relation to either of the organic dominions. This has been forcibly, as well as beautifully and justly, expressed by my learned friend and colleague, Professor Draper, in his able work on the *Organization of Plants*. Thus—

"As we have said, it is the character of a true theory to be applicable to all cases, and to render a clear account of every circumstance that may arise. A true theory is like a window of crystal glass, through which we can see all objects in their proper position, and colors, and relations, no matter whether they are such as are near, or those that are at a distance; no matter whether they are directly before us, or enter only obliquely into the field of view. A fictitious theory is like a Venetian blind, which has to be set in a certain position with respect to the observer, and only shows him objects for which it has been adjusted, and these in an unsatisfactory manner; but if he moves to one side or the other, or endeavors to see objects which are not directly in his way, his view is intercepted, or, perhaps, unless he makes a new adjustment, the light is shut out altogether."

In the first place, then, it appears to me that the hypothesis contains a fatal element—the prodigious amount of force which is said to be generated in the leaf, as well as the lungs and other soft structures of animals. On *this point* I am bound to abide by the decision of the chemists, who say that such *must* be the consequence of the chemical changes which are supposed to be in progress for the production of motion. As expressed by these philosophers, who designate it as "an *inexpressible force*," it would be abundantly sufficient for any purposes in artillery or in blasting rocks; and I infer, therefore, would hardly be withstood by the leaf or the lungs.

In the next place, there are many other circumstances attending the circulation of plants, as well as animals, which it would not be easy to interpret by the chemical doctrine, but which are readily explained by the vital. Such, for example, as the remarkable vigor of the circulation in the *acer* *saccharinum* before there is a development of the bud. Indeed, the harvest of maple-sugar often takes place in the Northern States while the earth is covered with snow to the depth of many feet. The circulation, too, is most vigorous after frosty nights succeeded by warm mornings; and when the temperature of the air for a night or two rises to some 40° F. the flow of sap is often nearly or quite suspended, but restored in profusion on the return of frost. What in chemistry will explain such a phenomenon? And if it retreat before obstacles of this nature, must it not abandon the whole ground? Nay, how palpable the force of a single fact, when it is considered that the phenomenon is due to the effect of heat as a vital agent on the irritability of vegetable life; and whether operating at the higher and more uniform degrees, or alternating at the freezing point, the exact explanation is involved in the law of vital habit, as set forth in the *Institutes of Medicine* at page 363—370.

Such, mainly, is also true of the vine, which was the subject of many experiments by the celebrated philosopher Dr. Hales, as appear in his *Vegetable Statics.* These experiments are allowed to have been ably and critically conducted, and are standard references. Let us, therefore, interrogate some of these experiments, and see how far they correspond with Nature, or how far they contradict her and bear out the chemist. Now, in some of his experiments there was not only an absence of leaves and buds, but the stump alone was the subject of observation. There was wanting, therefore, the fundamental requisite of the chemist, and, indeed, I may say, what is considered indispensable by all the physical philosophers to the simple doctrine of capillary attraction as it regards the ascent of sap. Take as an example *Exp.* xxxvi. Thus—

" April 6th, at 9, A. M., I cut off a vine on a southern aspect, two feet nine inches from the ground. The remaining stem had no lateral branches. It was seven eighths of an inch in diameter. I fixed on its top the mercurial gage;" of double curve to admit the flow of a few inches of sap.

For several days the mercury was more or less pushed up by the sap, according to the state of the weather.—" April 14th, at 7, A. M., the mercury rose to 20 inches high. At 9, A. M., 22 inches. Fine warm sunshine. Here we see that the warm morning gives a fresh vigor to the sap."—" April 18th, at 7, A. M., mercury 32 inches high, and would have risen higher if there had been more mercury in the gage. From this time to May 5th, the force gradually decreased [the life of the plant giving way]. On the 18th of April the force of the sap was equal to 36 feet height of water.

"Here," the Doctor concludes, "the force of the rising sap in the morning *is plainly owing to the energy of the root and stem.*"

In another and similar experiment, at the same time ("the mercurial gage being fixed *near the bottom* of a vine) the mercury was raised by the force of the sap 38 inches, equal to 43 feet+3 inches+⅓ height of water; which force is near five times greater than the force of the blood in the great crural artery of a horse; seven times greater than the force of the blood in the like artery of a dog; and eight times greater than the blood's force in the same artery of a fallow doe"—as ascertained by the rise of the blood in long glass tubes.

In these experiments it is sufficiently manifest that all the physical hypotheses fail, since all of them assume that the leaf or its equivalent is indispensable to the progressive rise of the sap. The result, I say, shows, what the observation of all organic nature teaches, that so important a function as the circulation, and so exceedingly variable as in plants, yet most exactly suited in every species and every individual, but varied in all the species, to the methodical steps in vegetation, is not left to the capricious operation of any chemical or physical agencies, and that a force is established at the very base of a plant, that shall not fail of the exigencies of vegetable life according to its progressive changes. It follows, therefore, that the sap is moved by something peculiar to living beings, and this is called a vital action. The motion which we have seen, however, would prove utterly destructive to the leaf, and even to all delicate branches, without a gradually countervailing intervention of that action, since the subdivision of vessels will not explain the diminution of force. I hence infer, what is denoted by other important facts, that the reduction of force arises, also, from a modified action in the vessels leading to the bud, as well as in the bud, or leaf, itself. Here a new action is set up, and a new motion of the sap begins, which is propagated along its downward course by a universal action of the vascular system, modified in different parts according to the special final causes of each part. Nor is it true, as supposed by one of the ablest chemists of the day, that, "Physiologists have here stepped in with their phantom Vitality, and *explained* the descent of the elaborated sap on visionary hypotheses, *that it was alive, or had obtained some vital qualities.*"

There having been no leaves in the foregoing experiments, and, indeed, only a short stump of the vine, the results were not unexpected to the philosopher, who adopts the theory that the circulation of sap is owing to temperature. But temperature would not always explain the phenomena, capillary attraction was little understood, and chemistry was yet unfledged. Accordingly, as in all cases where genius departs from Nature, even the acute mind of Dr. Hales has a special hypothesis for each apparent difficulty; sometimes borrowing from the theory of the vitalist, and actually raising hypotheses in direct opposition to each other. Take the following examples where the leaves had obtained their full development. Thus—

"July 4th, at noon, I cut off within three inches of the ground, another vine on the south aspect, and fixed to it a tube seven feet high, and filled it with *water*, which was imbibed by the root the first day, *at the rate of a foot in an hour*, but the next day much more slowly; yet it was continually sinking, so that at noon day I could not see it so much as stationary;" the life of the stump now giving way.

Here are two important facts. There was no apparent upward force, though there may have been some mingling of the sap with the water; and, secondly, the water being vitally adapted to the plant, it was actually carried down to the roots, at the rate of a foot an hour from the tube. There was no chemistry here to effect or in any way influence the descent. The pressure was also slight, and the hypothesis of the laboratory probably scouts the notion of gravity. Besides, I may say, if gravitation effects the descent, it should *prevent* the ascent. This experiment, like the preceding, appears to be decisive against our rival friends, and in a more important aspect; for the *descent* of the sap had been a greater problem to them than its *ascent*. The importance and compass of the proof will be at once perceived. But he who made the experiment, seeing the want of agreement with the preceding, fancied, like a great many other philosophers, that a conflicting fact would justify a special hypothesis. Let us, therefore, hear the Doctor upon this troublesome point. Thus—

"Now, since the flow of sap ceased at once, as soon as the vine was cut off the stem, the principal cause of its rise must, at the same time, be taken away, viz., the *great perspiration of the leaves*."

That is the doctrine, along with capillary attraction, of a large section of the physical school. But it arises from a defective observation of facts and from an ignorance of the difference in the physiological condition of the vine before and after leafing. Indeed, how can any *such* distinction be appreciated by those philosophers? In the experiments first recited, the vine was in its budding, and, therefore, bleeding season;

in the present, the leaves had become developed ; and notwithstanding the lifting power ascribed to them by the chemist, and that from the evaporation imputed by others, the circulation is transcendantly greater in the budding season, and Hales might have amputated the largest limb, with all the other leaves remaining, and the same descent of the water would have occurred, and prompted a different hypothesis.

And now contrast the foregoing experiment with his conclusion as expressed in *Exp.* xxxviii. ; the words in Italics being designed by myself to facilitate the hasty reader. It is a hypothesis, directly opposed to the preceding, for the purpose of expounding another fact.

" The sap," says the Doctor, " begins to rise sooner in the morning in cool weather than after hot days ; the reason of which may be, because in hot weather much being evaporated, it is not *so soon* supplied by the roots as in cool weather, *when less is evaporated.*" In *Exp.* xlvi. he says, " it was found that the trunk and branches of vines were always in an imbibing state *caused* by the *great* perspiration of the leaves, *except in the bleeding season.*" At that season the problem of the stump led him to conclude that " the force of the rising sap is plainly owing to the *energy* of the root and stem." (See *Exp.* xxxvi.) Will the chemist explain ?

In one of his experiments he attributes an effect to the " sun's warmth " in making the vessels " *dilate and contract a little.*" This is what he means by " the *energy* of the root and stem." Had he adhered to that explanation, he would have had no difficult problems to expound, no conflicting experiments, no contradictions of himself. Few philosophers, however, have been as accurate in their experiments, and few better qualified to reason upon facts, than Dr. Hales ; and what may we not, therefore, conclude of the reasoning of those who have assumed spurious inferences in vegetable physiology as a groundwork for the circulation and other organic processes in animals ?

Thus it ever is with all who depart from their main field of operations to build up the difficult parts of other sciences. Hales was a divine, and although adroit in experiments, and better qualified by impartial habits than the chemist, it is no detraction from his (or their) exalted merits to say, that he knew so little of physiology he was incapable of applying or even perceiving the facts which the student of organic nature may readily seize and convert to the philosophy of life, and turn against the conclusions of the original observer.

Am I not, therefore, entitled to conclude, that organic beings are contra-distinguished from inorganic by what is popularly known as life, or vitality, and with the summary remarks of the greatest scientific botanist of the age, of one who has devoted his life to the subject, Professor Lindley of the London University, as expressed in his able analysis of the "First Principles of Botany," that—

1. "The movement of the sap depends upon a vital irritability, and is independent of mechanical causes."

2. "The proximate principles are formed by the vital powers of the plant acting, in conjunction with air and light, upon the fluids contained in its system."

3. "*All the phenomena connected with the growth of plants are caused by an inherent* VITAL ACTION."

New York, Sept. 25, 1848.

CONTRIBUTIONS IN PHYSIOLOGY BY DR. PAINE.

[Communicated for the Boston Medical and Surgical Journal.]

NOTE *to the "Institutes of Medicine,"* §350.—PROFESSOR LIEBIG'S PHYSIOLOGY.—In my communication of Sept. 25 (vol. 39, p. 209), I stated Professor Liebig's summary doctrine relative to all organic and animal motions; namely, that—

"The CAUSE of the state of MOTION is to be found in a series of changes which the food undergoes in the organism, and these are the results of processes of decomposition, to which either the food itself, or the structures produced from it, or parts of organs, are subjected."

That, I remarked, is his combustive doctrine of life; and, with the exception of its palpable contradictions by other doctrines, it is carried out in all parts of his celebrated work on "*Animal Chemistry.*" Indeed, the hypothesis is often repeated; thus—

"In the animal body, we recognize as the *ultimate cause of all force only one cause*, the *chemical action* which the *elements* of the food and the *oxygen* of the air mutually exercise on each other. The only known ultimate cause of *vital force*, either in animals or plants, is *a chemical process.*" And again—"*All vital activity* arises from the mutual action of the *oxygen* of the atmosphere and the *elements* of the food."

I see, now, that we are presented in a work just published by the author of the foregoing doctrine ("*Researches on the Chemistry of Food, and the Motion of the Juices in the Animal Body,*" and re-published under the patronage of English and American Professors), with the following interpretation of the same phenomena. It will be seen that it is nearly the common doctrine relative to the evaporation by leaves in explaining the circulation of sap, as propounded by Dr. Hales, and as set forth in my article referred to above. The Professor infers the principle from experiments made upon *dried* membranes! Having found the membranes pervious to water, oil, &c., he proceeds to say, in a letter to Professor Horsford, re-published in the *American Journal of Science and Arts* (May, 1848, p. 415), and which I quote for the brevity of the conclusion, that—

"The employment of these results upon the processes in the animal body, scarcely requires a more detailed explanation.

"The surface of the body is the membrane from which evaporation goes constantly forward. In consequence of this evaporation, all the fluids of the body, in obedience to atmospheric pressure, experience motion in the direction towards the evaporating surface. This is obviously *the chief cause* of the passage of the nutritious fluids through the walls of the bloodvessels, and the *cause of their distribution through the body.* We know *now* what important function the skin fulfils through evaporation"!

Now, it is hardly necessary to say, in refutation of the foregoing hypothesis, that it is not only founded upon experiments upon dead matter, and ascribes all the processes of life to the merest physics, but it isolates the functions of the skin from all the other secretory processes, whose effect would be the same as imputed to the skin, if evaporation and atmospheric pressure were "the chief cause of the passage of the nutritious fluids through the walls of the bloodvessels, and the cause of their distribution through the body." The straining off of bile, urine, saliva, &c., and even of the "nutritious fluids," would be exactly the same in effect as perspiration; for it is unimportant whether the products escape into the air or go into the bladder, &c. The pressure of the atmosphere, therefore, would be equally balanced throughout, and amount to nothing.

That the reader may see, at a glance, how far the distinguished chemist is also a physiologist, and for the benefit of his numerous admirers, I will close this notice with an extract from the author's work on *Animal Chemistry*, which, it will be seen, is near a-kin to the foregoing. Thus—

"If we consider the fatal accidents which so frequently occur in wine countries from the drinking of what is called feather-white wine, we can no longer doubt that GASES OF EVERY KIND, whether soluble or insoluble in water, *possess the property of permeating animal tissues, as water permeates unsized paper.* This poisonous wine is wine still in a state of fermentation, which is increased by the heat of the stomach. The carbonic acid gas which is disengaged permeates through the parietes of the stomach [!], through the diaphragm [!], and through all the intervening membranes [!] into the air-cells of the lungs [!], out of which it displaces the atmospherical air" !!!

Such is only a common example of the physiology of a most able chemist, and it derives its importance alone from the eclat with which his physiological writings have been received wherever science has been cultivated, and, I may say, wherever it has not been. The consequences, therefore, are not merely such as arise from idle words, but they seriously vitiate the whole fabric of theoretical and practical medicine.

Note to §18; 42; 409; 419.—SECRETION.—As a consequence of the doctrine that the secreted fluids exist in the blood, as "constituent parts," it is assumed that they are habitually strained off (as we have seen in the foregoing extracts from Prof. Liebig), and that when an organ fails of its appropriate office, its secreted product may escape from the blood by way of another organ. "Sometimes," says Muller, "the suppression of a secretion in one part of the body gives rise to the appearance of the same fluid in another part." And again, "The sole secretion, of which the *constituents* do not exist *as such* in the blood, but which can, nevertheless, be formed at all times and in all parts of the body, is *pus*; the organ for its production being generated anew in the process of inflammation." (Muller's Physiology, vol. 1, pp. 474, 475, *First Edition.*)

The latter quotation is omitted in the *second edition* (p. 520), perhaps from the late reputed discovery of pus-globules in the natural blood;

and this eminent physiologist supposes that *perfect* milk cannot be eliminated by any other organ than its appropriate gland. There occurs, also, the following general qualification—"If, however, the essential ingredient of the secretion does not exist in the blood itself, the suppression of this secretion in the organ destined to form it cannot cause its metastatic appearance in other parts. The instances which have been adduced of such an occurrence are ill supported by proofs." And, again, he says,—"The formation of any one of the peculiar secretions, the essential proximate constituents of which do not exist in the blood itself, pre-supposes the operation of *a special chemical apparatus*, whether this be a membrane or a gland." "A part of the *liquor sanguinis*, with the matters dissolved in it, is imbibed by the tissues (*by endosmosis*), by the agency of which it undergoes a chemical change." And yet he says—"The chemical process of secretion *is not at all understood.*" Muller finally yields to the philosophy which about balances the chemical, in his great work on Physiology, and argues that—"The nature of the secretion depends, therefore, SOLELY *on the peculiar vital properties* of the organic substance which forms the secreting canals, and which may remain the same, however different the conformation of the secreting cavities may be; while it may vary extremely, although the form of the canals or ducts remains unchanged."—(*Ibid.*, *Second Edition*, pp. 429, 474, 510, 511.)

Now the question arises, whether all these conflicting doctrines upon one and the greatest topic in organic life, put forth by the ablest physiologist of our time, can possibly be true; and if not, which are we to elect, the *mechanical*, the *chemical*, or the *vital*? Each one stands forth conspicuously in the work from which I have quoted. The mechanical and chemical are surrounded by conjectures, contradictions, and admissions of absolute ignorance of their nature and philosophy, while the vital is alone consistent, is isolated from the others, is extensively concerned with the nervous power in animals but excludes it from plants, is expounded with admirable ability and in total opposition to the others. Whence comes this confusion in fundamental principles and laws? Organic Chemistry will supply an answer.

I began this article with a view of offering a single consideration against the mechanical hypothesis of secretion, which has now become so incorporated with the chemical, that the distinction is not readily appreciable but in instances like the present, where it is assumed that the "proximates" are about as numerous in the blood as the secreted products, and generally the same. Owing to the prevalence of this doctrine we have come to be quite familiar with the term *vicarious*, as "effusions of urine from the mammary gland *vicarious* of milk," &c.

Having examined extensively the whole of this subject at former times, it is my object now only to add some conclusions from a fact which appears to me sufficiently corroborative of the doctrines of life, and opposed to all others.

Chemical analyses of the blood inform us that its composition in males and females is the same, and there appears to be no reason to question the accuracy of a statement which is inferable from the identity of the chylopoietic and sanguiferous organs in both sexes. Upon this undoubted truth, therefore, if the "proximates" of milk, as affirmed, exist in the blood of females, they should be equally found in that of males, and the seminal fluid of the latter, or its "proximates," at least, should be present in the former. This will scarcely be maintained; nor can the period of gestation be set up in behalf of one, nor the difference of sex to expound the other, since this would be conceding the whole physiological doctrine, and therefore the fallacy of the mechanical hypothesis of secretion. If, also, there be nothing like the seminal fluid in the blood of females, and it requires a testis to generate that substance, by parity of reason, the gland which is peculiar to the female has been ordained for an equally absolute formation of milk, and for nothing else. We have, therefore, besides the simplicity and designs of Nature in any great organic plan, a special demonstration of the equal dependence of semen and milk upon common physiological laws, both in the nature and final causes of the products, and in the analogy which subsists between the organization of the two glandular organs, while each, respectively, is peculiar to the different sexes.

It must be readily seen, that this reasoning, with its analogy, is of universal application to all the organs and products of the body, and that if nothing like semen or milk exist in the blood, and it require a testis and a mammary gland to generate those products, it is equally true that there is nothing like urea, bile, gastric juice, or millions of other unique formations which go to the uses of the various parts of different species of animals, in the blood, and that a kidney, a stomach, a special organization for every special product, is as indispensable for the formation of each of the products as is the testis for the seminal fluid, or the ovarium for the germ; and the proof is as good against the imputed "vicarious" discharges of urine from the mammæ or from other parts, bile from the skin, &c., as the failure of these discharges from the testis is a proof that this organ is so constituted that it shall elaborate nothing but semen. Indeed, abstractedly from all the vast amount of proof which may be brought to the same conclusion, we might rest in the consistency of nature upon the simple analogy supplied by Muller, that pus has no existence in the blood, "*the organ for its production being generated anew in the process of inflammation.*" What, therefore, shall be said of the manufacture of gastric juice?

It is unnecessary to say that some distinguished chemists deny the existence of any of the "proximates" of urine, bile, or milk, in the blood; nor need it be added that if the product of an organ be absorbed into the circulation, and afterward excreted by the skin or kidneys, it is totally different from what is meant by a "vicarious effusion," and is on common ground with the habitual excretion of other redundancies of the blood. But we have no proof, as I have endeavored to show, that either the bile (except the coloring matter), or more than the watery parts of the urine, are ever absorbed into the circulation.

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